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SCIENTIFIC BEACHCOMBING*

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ALL through the August night, a light wind had fanned the waters of the lake. Sparkling in the moonlight, a continuous procession of small waves had marched in, only to break against the long, clean, sandy beach in front of the cabin. I had watched for several hours before turning in, and tomorrow, I had told myself, tomorrow I bet I'll find out. Conditions were just right. So, in the early morning I dressed hurriedly and started for the shore—hoping, as so many times before, to find some clue to the solution of the problem.

It was an old problem, this question of how beach cusps are formed, and had been the subject of discussion, study, and experiment by geologists and other nature students for more than a century. Many articles had been written and many theories advanced, by both laymen and scientists, yet the answer seemed as far away as ever.

And it was a tantalizing problem, almost maddening in its simplicity. The processes involved were few and well known, and the forming of the structures certainly took place in plain sight on the open beach. Yet, strangely, no man could say he had seen their building.

Such a problem becomes a game, filled with all the hopes, thrills, and disappointments of the hunt. You may follow the trail for days, using all the tricks and devices suggested by a long experience, without get-

Spence Air Photo

BEACH CUSPS[1]

WHY IS THIS SANTA BARBARA SHORELINE SCALLOPED?

* In "The Brownstone Tower" of the SM (April 1945) the editor called for stories of detection in which customary scientific reserve would be broken down and the drama of scientific discovery played up. "Scientific Beachcombing" is the first response to the editor's appeal. Previously Professor Evans published for specialists a detailed, conventional account of his discovery (The Classification and Origin of Beach Cusps, *Journal of Geology*, 46: 615–627. 1938); now he presents a part of the same story dramatized for laymen. The editor will be glad to consider for the SM other vitalized stories of research.—ED.

[1] This photograph was kindly provided by Dr. Martin A. Mason, Beach Erosion Board, War Department, Washington, D. C.

ting a glimpse of the quarry. Then there suddenly comes the elation of discovery after discovery.

But this was the most baffling of anything I had tackled in a lifetime of research. I used every known trick of the trade. I searched the literature, I made experiments, I haunted the beach for weeks. And from the illiterate fisherman to the erudite mathematical physicist, I pestered my friends with questions. But results were discouragingly few.

Despairingly, I almost gave up a dozen times. Sometimes I would think, "It is, after all, only a 'pure' science problem. No one's life depends on my finding the answer; neither will anyone, including myself, be the wealthier for its solution."

Yet I never quite gave up. In such times of discouragement, because I love the sea, and the sound of its waves quiets and soothes me, I was always drawn again to the beach. There, as I watched the waves, some new angle of the problem would always show up, and, with new enthusiasm, I would be off again on a new trail.

Thus for three months, since early in June, I had followed the sandy shores of the lakes, studying the wind, the waves, the currents, and the shoreline, trying desperately to wrest from nature the jealously guarded secret.

Thus, hopefully, yet with little real expectancy, I looked at the broad expanse of sandy beach and quiet lake that August morning. Thoughtfully, I strolled along, noting every movement of the water, every change in the shore, and every new combination of wave effects.

With increasing interest, I saw that during the night the waves had built a little ridge, two or three inches high, along the edge of the water. But there was nothing unusual about this one. It followed along just above the water's edge, without a break, except where a quick turn in the shoreline had prevented the waves from doing their work. There, instead of the ridge, was a gentle, even slope of water-washed sand, along which the little swashing breakers traveled.

Alert, through long practice, to every change in sound or movement of the water, my ears caught a changed note and timing in the murmur and plash. A new breeze was moving in from the lake, and the waves, outrunning it, were just reaching the shore.

Almost subconsciously, I noted that these new waves were a little higher and longer than those that had built the ridge. As the first one curled against the shore, water from its slightly uneven crest splashed, here and there, across the ridge's top. The next wave did the same, and by the time the third broke, it was certain the ridge was being divided into segments.

That was what I had been looking for! I watched, all attention, as the swash and backwash continued and the spacing of the breaks became more equal. To my astonishment, within a minute and a half the process was complete. A perfect series of beach cusps had formed. Indeed, had I happened along two minutes later, I would have supposed them to be many hours old.

The thing was so sudden, I stood for a couple of minutes gazing at the newly formed cusps, before the full significance of the event burst upon me. I had just witnessed a process in nature never before seen by human eyes; at least by any eyes that understood what it was they saw. My first impulse was to rush up and down the beach, shouting the good news.

But the inhibitions of a long professional life are strong. Besides, no one was in sight at that early hour, and I felt sure that those still asleep in the cabin would not be too appreciative of being awakened, only to be told that I had seen the waves splashing in a new way against the beach. Also, with some uneasiness, I saw that the wind was rising and that the waves might soon sweep the beach clean and smooth again. So I smothered my emotions and set to work at once with measuring tape and notebook.

Now, at last, I understood why the solution of the problem had eluded me so long. The delicate balance of factors entering into the process, combined with the rapidity of the action when the exact adjustment is reached, almost completely masks the operation.

The start of the process depends on the ability of the waves to break the ridge into segments. This they can do because of the

slight inequalities in the height of the wave crest. But if the waves of the new series are too large, the beach ridge, instead of merely being broken, is completely destroyed, but if they are not quite large enough to overtop the ridge, they just build it higher.

The breaks, at first, are spaced quite unevenly. But some are larger than others, and the parabolic currents of the swash and backwash have an equalizing effect on the spacing. Thus the larger breaks become the focal centers which determine the spacing as some of the smaller ones fail to develop.

And the process is remarkably rapid. A dozen waves, uniform in height and of the right size, are sufficient to complete the process and give the cusp series all the appearance of old age. And even now that I understand the process, this almost unbelievably delicate adjustment is still a source of astonishment.

Seven years have passed since that memorable August morning, yet never again have I seen the complete process of cusp formation go on to completion. Occasionally I see it start, then fail to finish because of some change in strength or direction of the wind. Also, I think I have been present a few times at the finish, although it is more difficult to be sure of this.

However, now that I know the process, I can perceive the truth or falsity of many of the beliefs held by former students. Sometimes the cusps build along a low cut bank; its edge acting as the crest of the beach ridge, and I have become convinced, through examination of hundreds of cusp series, that they form only when some sort of ridge is present; never when the beach slopes uniformly up from the water to beyond the limits of wave action.

The cusps I saw forming were perpendicular to the shoreline, but I have discovered, by many anxious days of shore watching, that if the adjustment between waves and ridges is sufficiently close, cusps may be formed with their axes at almost any angle to the shoreline. And it is still more surprising that a change in wave direction sometimes brings a change in this angle without destroying the cusps, although under slightly different conditions they will be destroyed

VARIOUS BEACH CUSPS[2]
MANY VARIATIONS IN FORM HAVE BEEN OBSERVED.

almost instantly. Thus, on a north and south shore, a cusp series built by a west wind may be changed by a southwest wind to look like one built by a wind from the northwest. No wonder speculative observers have given so many conflicting accounts of what they saw!

A constant source of surprise is the variety in size, form, and spacing of the cusps. Some are long and slim, others short and broad. Some have sharp crests and steep sides, others are low and rounded. And they vary in size from little scallops to a series of veritable capes.

Perhaps, for the average observer, the most striking feature is the apparent uniformity in spacing, which has even led some scientific observers astray. Somehow, symmetrical regularity of feature seems out of place on a beach, and maybe the unconscious comparison induces a sort of optical illusion.

Careful measurements of my cusp series gave a spacing of 1.7, 1.6, 1.6, 1.9, 1.4, 1.0, 1.3, 1.6, and 1.4 feet. Thus the variation in spacing was 90 percent although, at first glance, it seemed almost uniform. In measuring larger cusp series, some of which had a spacing of over 20 feet, I found variations ranging from 40 to 147 percent.

However, with the finish of my field observations, my troubles were not over. The question at once came up, "What is a beach cusp?" Going back again to the literature of the subject, I found that a beach cusp is

[2] Reproduced by permission from page 465 of the late Douglas Johnson's *Shore Processes and Shoreline Development*, John Wiley & Sons, New York.

defined more by inference than by direct statement. It is clear that beach cusps are so named because of their shape. They are merely points extending out into the water. Thus it seems that any point of sediment, more or less temporary in nature, sticking out into the water from the shore, has a right to be called a cusp.

Now it is perfectly certain that not all such points of sand are formed in the way I observed, and not all of them occur in series. So, because of looseness of established definition, I was forced to make a classification of beach cusps, including some forms that I do not really think belong there. Thus, as is not unusual, I was forced in my scientific writing to defer somewhat to the generally accepted, though sometimes erroneous, statements of earlier writers.

"But now you've found this out, what's its value and how can we use it?" you inquire; a fair question, and one that could well be asked at the finish of any investigation.

Here is your answer. In the long history of the earth, the sea has always been advancing and retreating across the continents. Sometimes this movement is relatively rapid, again it is very slow. As it goes on, many shore structures are built, of which the beach cusp series is one. And sometimes such structures are solidified and preserved in formations like the Medina or the Potsdam sandstones. Finding such a fossilized beach cusp series on an old shoreline and knowing how it was made, we can say what was the direction of the wind, the size of the waves, and something about the shape of the beach on that far-off day in the earth's history. And so we are brought another step nearer to knowing how this old earth was made.

OREN F. EVANS

OREN F. EVANS, Ph.D., is Professor of Geology in the University of Oklahoma. He writes:

"The one thing I have in common with many of the famous men of my generation is that I was born in a log cabin. That most important event occurred on a June day 67 years ago in a clearing in the woods a few miles from Shelby, Michigan.

"That country was pretty new and crude in those days, but I have sometimes thought that many of the things I learned, as a pioneer boy, among the hills, woods, and lakes of Michigan have been as valuable to me in my work as a field geologist and teacher as a lot of the more formal training that came later from the schools.

"I began teaching at an age when, according to modern standards, I should have still been in high school. I taught for a number of years, going to school between times. As a result, my college education was considerably broken, and because of changing conditions and changing desires, I was a student at four different institutions of higher learning: Purdue University, Michigan State Normal College, Albion College, and the University of Michigan. A few years after graduation, I was called back to Albion College, where I taught mathematics and physics two years. In 1920 I joined the faculty of the University of Oklahoma, where I am still working as a Professor of Geology.

"My summers, for many years, have been devoted to physiographic research, mostly along the line of shore processes. I have been a contributor to the *Journal of Geology, Science, American Journal of Science,* and *Journal of Sedimentary Petrology* as well as having written a number of semi-popular scientific articles." In one of his letters Dr. Evans added: "For a number of years, I have been pondering this matter of popular scientific writing and finally decided to do something about it, at least in my own science. So for two years now, I have been training under two of my author friends, Stanley Vestal (Professor W. C. Campbell) and Foster-Harris. This accounts for the techniques of the fiction writer which I used in 'Scientific Beachcombing.'"

SAFEGUARDING OUR SEAWAYS—THE MODERN NAUTICAL CHART

By A. L. SHALOWITZ
PRINCIPAL CARTOGRAPHIC ENGINEER, U. S. COAST AND GEODETIC SURVEY

To all peoples whose territories touch the sea or who have any interest in the commerce of the sea a full and complete knowledge of the coast, its nature and form, the character of the sea bottom near it, the locations of reefs, shoals, and other dangers to navigation are of the greatest practical value. Modern nautical charts supply such information.

Of the total area of the earth's surface the oceans occupy nearly three-fourths, affording highways open to the nations. To conduct international commerce by water the ships of one country must enter the ports of another, so that on the open sea and in the harbors there is an interest, common to seamen of all nationalities, in the publication of charts. Progressive maritime countries have therefore long recognized that one of their important political obligations is to survey their coasts and to publish charts showing the results of such surveys.

While many innovations have been introduced in the art of navigation, all of these would be of little value were the charts permitted to deteriorate in accuracy. Radio bearings may enable the mariner to locate his position far out at sea, but he must have a chart on which he can accurately plot that position and determine his relationship to surrounding submarine features and to the topography of the land.

An important distinction exists between the nautical chart and maps in general. Whereas the latter may serve as reference mediums, the nautical chart in its special and accurate delineation is an instrument to be worked upon so that a ship's course may be laid off with accuracy and ease, and positions readily determined.

From Ptolemy to Mercator. Although the modern chart is of comparatively recent origin, the period from Ptolemy to Mercator saw three great developments in cartography that have profoundly influenced contemporary chart making. Claudius Ptolemy—Egyptian mathematician, astronomer, and geographer—who lived in the early part of the second century, stands without doubt in the front rank of early geographic thought. His *Geographia* represented the sum of all geographic learning and served as a groundwork for future cartographers. Ptolemy gave details for the construction of 26 maps and a general world map and is credited with being the originator of the conic projection and with being the first to divide the earth by means of meridians and parallels.

Another great development in cartography came towards the close of the middle ages and forms a notable exception to the prevailing darkness of that period. The Italian and Catalan chart makers produced what were known as The Portolanos, or "handy plans." These charts came into being generally with the introduction of the compass in navigation. No projection was included, but in its place there were networks of straight lines, each network radiating from a common center like the spokes of a wheel and corresponding to the points of the compass. The Portolanos achieved only an approach to mathematical accuracy, but it was enough to give the seamen of that period the confidence which they needed to sail the open sea. It remained of course for Mercator, 150 years later, to *solve* the problem of cartography for the navigator.

The influence of the Portolanos on chart making was felt for several centuries after their introduction, and Juan de la Cosa in 1500 still covered his chart with the spider-web lines (Fig. 1). Cosa accompanied Columbus on his first voyage as master of his flagship and as cartographer on his second voyage. The Cosa chart is of great interest historically, being the earliest map now extant which shows the American coast.

The third great influence on the modern nautical chart was the contribution of Gerhard Kremer (Mercator), the Flemish mathematician and cartographer who lived in the

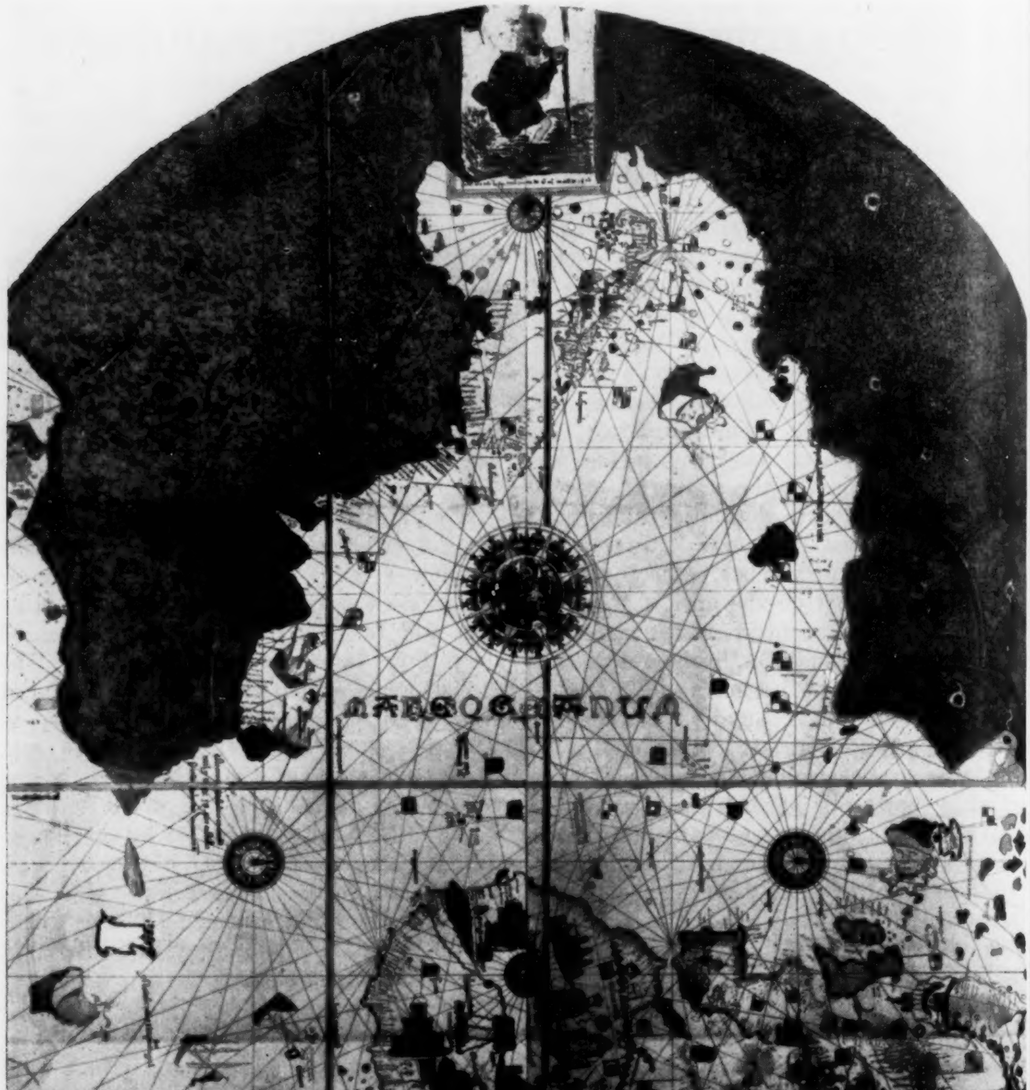
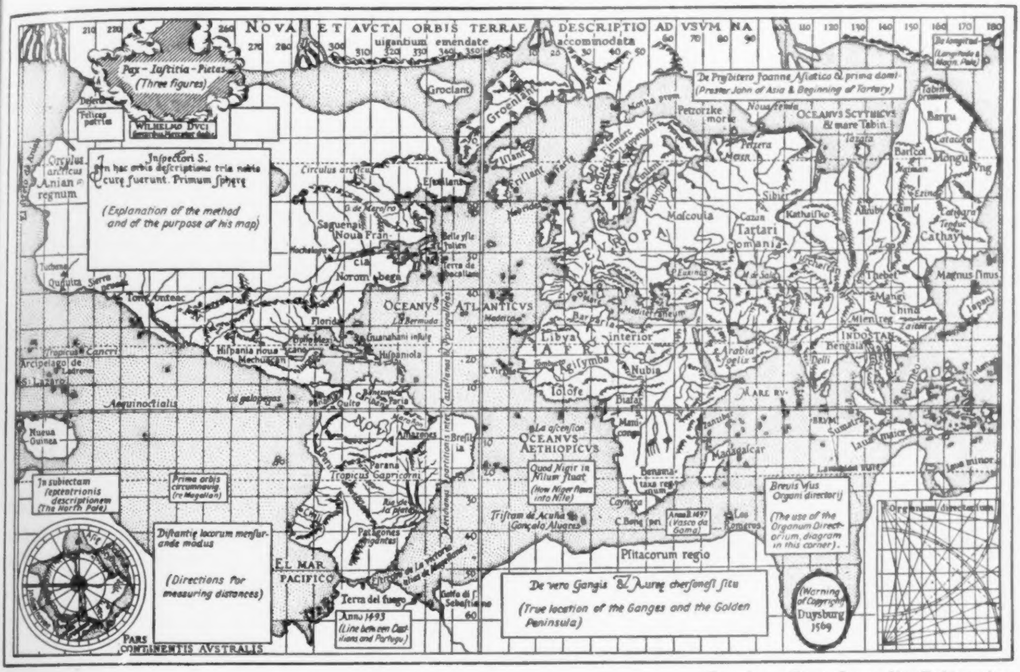


FIG. 1. CHART OF NORTH ATLANTIC OCEAN BY JUAN DE LA COSA, 1500

THE COSA CHART WAS DRAWN ON OXHIDE AND IN BRIGHT COLORS. THE RADIATING LINES, WHICH CHARACTERIZED THE PORTOLANOS, ENABLED THE NAVIGATOR TO SET HIS COURSE AT ANY POINT BY AID OF THE MAGNETIC NEEDLE. TO RECOGNIZE THE WESTERN COAST OF EUROPE AND AFRICA TURN THE MAP, RIGHT-HAND SIDE ON TOP (NORTH).



Courtesy Encyclopaedia Britannica

FIG. 2. MERCATOR'S WORLD CHART OF 1569

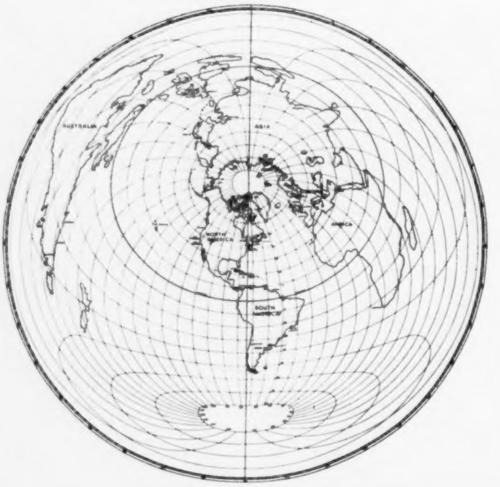
ON HIS ORIGINAL CHART MERCATOR ADVISED: "IF YOU WISH TO SAIL FROM ONE PORT TO ANOTHER HERE IS A CHART AND A STRAIGHT LINE ON IT, AND IF YOU FOLLOW CAREFULLY THIS LINE YOU WILL CERTAINLY ARRIVE AT YOUR PORT OF DESTINATION. BUT THE LENGTH OF THE LINE MAY NOT BE CORRECT YET IT POINTS IN THE RIGHT DIRECTION. CONSEQUENTLY IF YOU ADHERE TO THE LINE YOU MAY GET TO YOUR DESTINATION SOONER OR YOU MAY NOT GET THERE AS SOON AS YOU EXPECT, BUT YOU WILL CERTAINLY GET THERE."

sixteenth century. Ptolemy, as we saw, introduced the latitude and longitude idea in maps. The Portolano chart makers of the fourteenth century neglected this concept and used the points of the compass as *their* "grid system." Mercator combined the scientific theories of the one with the practical advantages of the other and devised the well-known projection which bears his name. In his famous "World Map of 1569," the latitude and longitude lines are straight parallel lines intersecting each other at right angles (Fig. 2). The meridians of longitude Mercator spaced equally throughout his map, based on their distance apart at the Equator. This, of course, caused a spreading of the meridians everywhere except at the Equator, since meridians on the earth converge toward the Poles. To compensate for this, Mercator conceived the idea of also spreading the parallels in exactly the same proportion as he spread the meridians.

What Mercator sought to accomplish by this arrangement of meridians and parallels was to provide the navigator with a chart on which a straight line—the simplest of all lines—joining any two points would determine the course he must steer in sailing between those points. Such a line is known as a *rhumb line.* On the earth it cuts all the meridians at the same angle and is a continually curving line, always approaching the Pole but theoretically never reaching it. A ship sailing a "rhumb" is therefore on one course continuously. The uniqueness of the Mercator projection lies in the fact that on it and it alone the rhumb line is a straight line.

Although it took nearly a century for navigators to appreciate this property of the Mercator projection, today the projection is looked upon as one of the most useful ever devised for marine navigation and will likely be so considered as long as ships "sail the rhumb."

There are, of course, certain limitations



Courtesy General Electric Company

FIG. 3. AZIMUTHAL EQUIDISTANT MAP
DISTANCES AND DIRECTIONS ARE CORRECT FROM SCHENECTADY, N. Y., THE CENTER FOR THIS PROJECTION.

inherent in the Mercator projection which should not be overlooked. For example, a great circle—the shortest distance between two points on the surface of the earth—is a curved line on the projection. This means that radio bearings, which follow the paths of great circles over the earth, cannot be plotted as straight lines on a Mercator chart without correction. Also, as one recedes from the Equator, both north and south, the scale of the projection continually increases, reaching infinity at the North and South Poles. This expanding scale makes the projection unsuitable for charting the polar regions and limits its use for ordinary geographic purposes where equal-area representation is desirable.

But notwithstanding its limitations, Mercator's contribution will always remain one of the great landmarks in the development of nautical cartography.

Much criticism has been directed against the Mercator projection—particularly in recent years with attention focussed on long-distance aeronautical flights—because of its failure to embody some of the properties of other projections. In fairness to Mercator it should be stated most emphatically that for world mapping the "projection to end projections" has not yet been devised—nor will it ever be—for the simple reason that one cannot flatten the surface of a globe without distorting it. And as long as the surface is distorted, the component parts of the earth cannot be represented in their true relative sizes, shapes, locations, and directions. Any map projection is, at best, a compromise, and the choice of projection usually depends upon the purpose which the map or chart is to serve.

One hears much these days of the "azimuthal equidistant projection" as though it were the fulfillment of a cartographer's dream. A glance at Figure 3 will show how erroneous this is. Note, for example, the distortion in both shape and size of Australia. In reality if Australia were superimposed on the United States it would roughly coincide with it. This projection, however, does possess the useful properties that straight lines radiating from the center represent great circles in their true azimuths from it and that distances along these lines are true to scale. But these properties do not apply to other portions of the map. In the Mercator projection, the straight rhumb line is the essential property to be preserved and other properties are subordinated to it. To preserve this property Mercator introduced the distortion in the higher latitudes. To consider this distortion as a weakness of the projection is to overlook completely the purpose for which it was devised.

The Development of Systematic Surveying. The great explorations which followed the discovery of the New World added immensely to the meager geographic and hydrographic knowledge of the times, and the information thus acquired was compiled both in chart form and in the form of sailing directions. But these were largely the contributions of individual effort and private undertaking. The results obtained were in many cases commendable and meritorious, but in others there was much to be desired. The early chart of New York Harbor is an example of the charts of that period (Fig. 4). This chart, like all of the early maps, suffered from two great defects—the want of detailed surveys and the lack of a rigid system of connection between the various ports.

It was not until the early part of the nineteenth century that governments began to

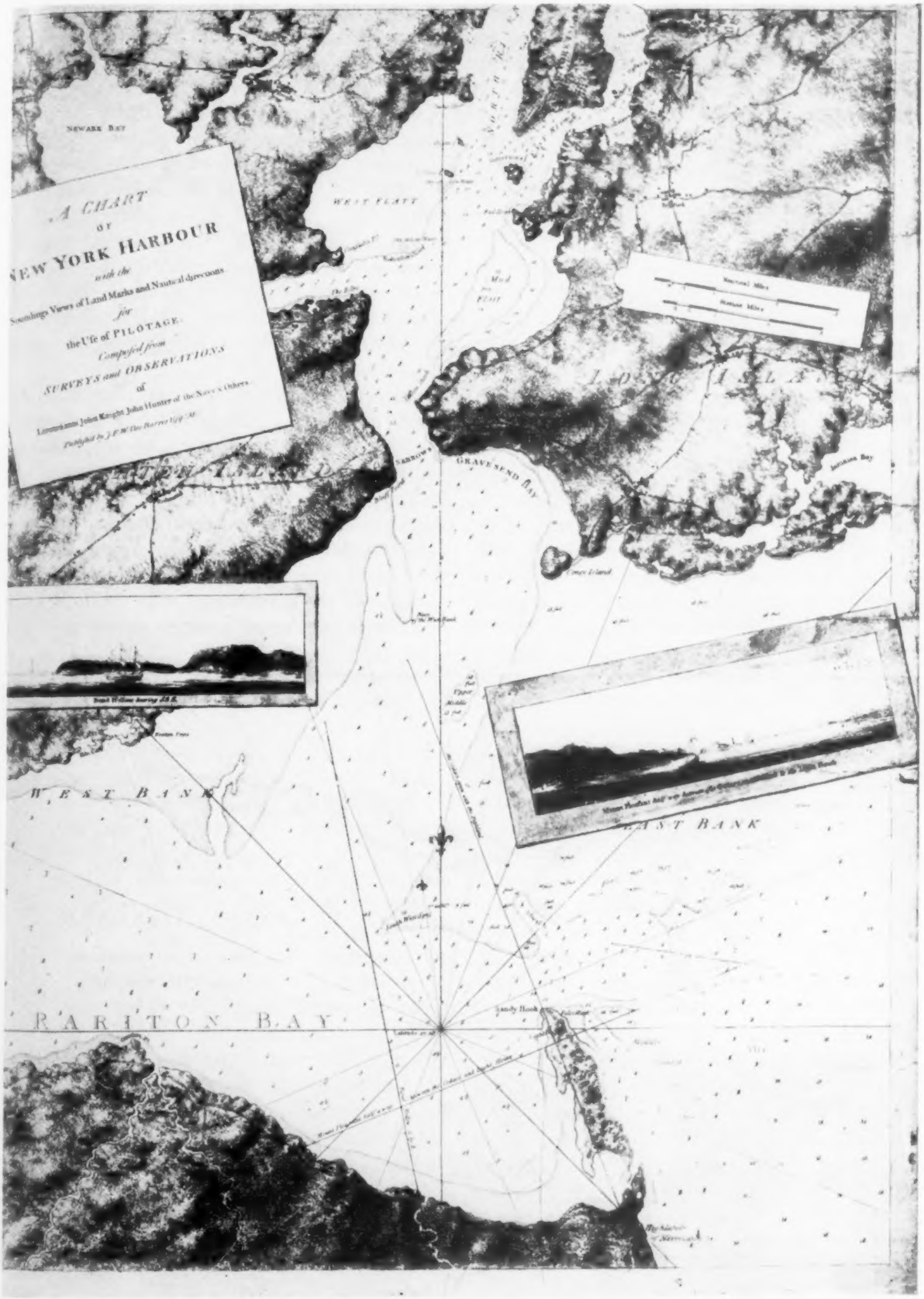


FIG. 4. AN EARLY CHART OF NEW YORK HARBOR

FROM THE ATLANTIC NEPTUNE COLLECTION, AN ATLAS OF CHARTS COMPILED BY DES BARRES, 1775 TO 1781.

recognize the wisdom of systematically surveying and charting their coastal waters as a necessary prelude to their commercial intercourse with other nations. It marked a new era in chart making and was the beginning of the accurate chart of today. In this country, the Coast Survey was created in 1807 to survey and chart the coastal waters of the United States, although actual work was not begun until a much later date.

The first chart published under the new governmental set-up was of Newark Bay, New Jersey, in 1835. This chart was a striking contrast to anything that had previously been called a chart of our coast. The outstanding improvements were high accuracy of geographic position, more thorough hydrography, and complete topography.

Charts of this period were entirely engraved on copper. Every line, every figure, and every letter was engraved by hand. The fineness of detail that was possible by this method of reproduction afforded the chart engraver an opportunity for artistic expression seldom equalled in any other method.

Many of the early charts had their margins adorned with elaborate views of harbor entrances and headlands for the guidance of the mariner, one of the finest of these being the view of Anacapa Island (Fig. 5) which was engraved by James McNeill Whistler, while employed in the Coast Survey. Whistler's stay in the Survey was brief but hectic. The rules of the office were soon found too exacting for his artistic temperament, and he became a habitual late-comer. When chided about it in later years he always replied, "It was not that I arrived too late in the morning, but the office opened up too early."

Since the publication of the first chart by the Coast Survey, the chart has undergone changes both in character and appearance, resulting mainly from improvements in methods of surveying and of reproduction. Physics, mathematics, the lithographic arts —all contribute toward producing a chart that the navigator can use with confidence.

Some of the more significant changes have been the adoption of a single depth unit for

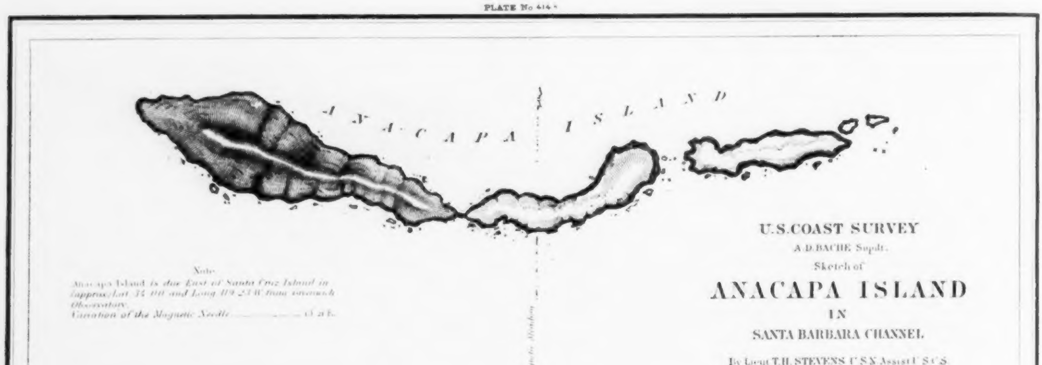


FIG. 5. ANACAPA ISLAND, CALIFORNIA

FROM AN ENGRAVING BY JAMES McNEILL WHISTLER, DONE WHILE HE WAS EMPLOYED BY THE U. S. COAST SURVEY.

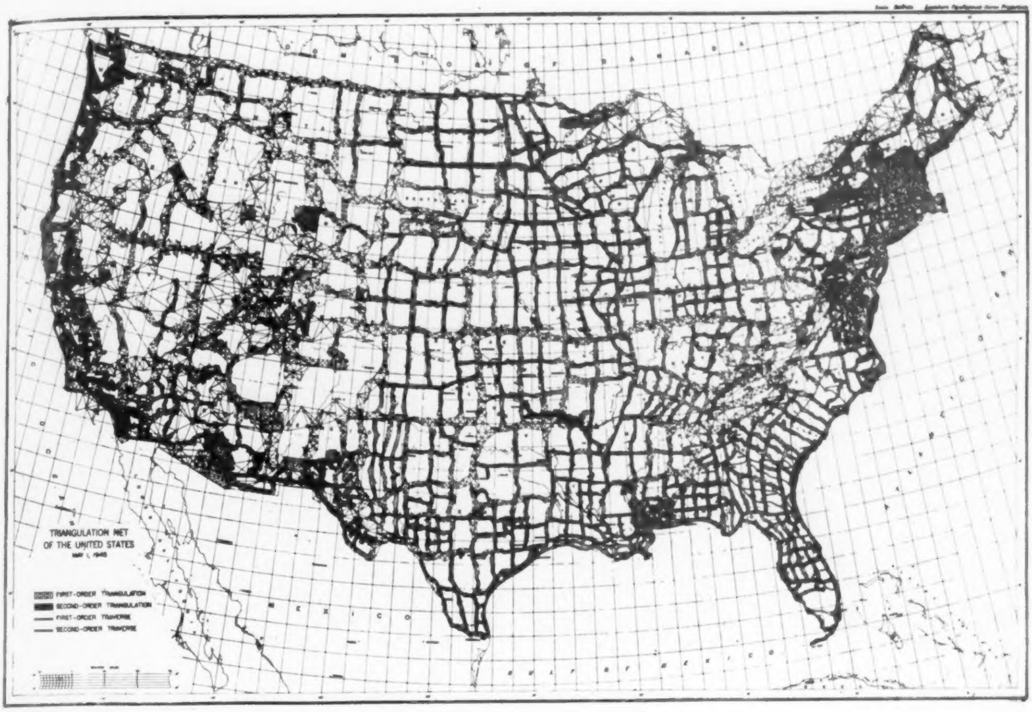


FIG. 6. TRIANGULATION NET OF THE UNITED STATES

97,000 LINEAR MILES OF FIRST- AND SECOND-ORDER TRIANGULATION FURNISH A RIGID FRAMEWORK FOR THE MAPS AND CHARTS OF THE COAST AND GEODETIC SURVEY, AND PROVIDE STARTING POINTS FOR LOCAL SURVEYORS.

any given chart to avoid confusion to the navigator; enlargements of the depth figures and the compass rose; the replacement of hachures by contours for showing topographic relief; and the adoption of a standard style for the nomenclature—vertical lettering for the land features, slanting letters for hydrographic features (Fig. 16).

The introduction of photolithography in chart reproduction has made it possible to use colors for emphasizing important navigational features. For example, the coloring of buoys to correspond to their colors on the ground; the accentuation of lighted aids to navigation by using a color overprint; and the use of tints for the land and shoal-water areas. The nautical chart of today is a synthesis of the utilitarian and the artistic, and is suitable for meeting present-day demands for quality and quantity production. Today each rotary lithographic press in the Coast Survey rolls off 3 to 5 thousand impressions an hour, as compared with the hundred-a-day maximum possible by the older method of printing directly from engraved copper plates.

Good and reliable charts can be made only from correct surveys. No one appreciated this more than Professor Hassler, the Swiss engineer to whom President Thomas Jefferson entrusted the important work of directing the survey of the coast. From the very beginning, Hassler insisted that all the harbor and coastal surveys be controlled by a backbone of triangulation (Fig. 6) that would knit together into one harmonious whole the surveys and charts of one locality with the surveys and charts of other localities. Notwithstanding the criticism heaped on him by certain politically minded gentlemen of the day, who could see no need for elaborate base measurements and triangulation systems, Hassler's scientific approach prevailed. And this tradition of accuracy which was inaugurated by him has been steadfastly maintained through a century

FIG. 7. 116-FT. TRIANGULATION TOWER
TO OVERCOME INTERFERENCE IN VISION DUE TO THE EARTH'S CURVATURE, FOREST GROWTH, ETC., THE OBSERVING INSTRUMENT MUST BE ELEVATED ABOVE THE GROUND. THIS STEEL TOWER IS PORTABLE AND CAN BE ERECTED OR DISMANTLED IN A FEW HOURS.

and a quarter of progressively increased activity.

Continuous, accurate triangulation has now been extended from the lower end of Mexico through the United States, Canada, the Yukon Territory, western Alaska, to the Little Diomede Island in Bering Strait. All of this work is based on a single geodetic datum, the North American datum of 1927, the initial for which is Station Meades Ranch in central Kansas. The establishment of this datum has resulted in the complete coordination between nautical charts of the Atlantic and Gulf Coasts and those of the Pacific Coast and Alaska—a most enviable position for any country to be in.

Improved instruments and equipment have accelerated the progress of triangulation work. When Hassler first started his work in 1816, he had a special carriage built to transport his great cumbersome theodolite (24-inch circle). The theodolites used today in first-order work (9-inch circle) can be carried by one man. Night observing on electric signal lamps and the portable steel towers (Fig. 7) have also contributed to this progress.

An important feature of the nautical

FIG. 8. MAPPING AN ALASKAN COASTLINE WITH THE PLANETABLE
THE PLANETABLER CONSTRUCTS HIS MAP AS HE SURVEYS. THE RODMAN ON THE POINT OF ROCKS IS HOLDING A "TELEMETER" ROD AND THE OBSERVER IS MEASURING ITS DISTANCE AND DIRECTION FROM THE PLANETABLE.

FIG. 9. CONTACT PRINT FROM 9–LENS AERIAL CAMERA
AT AN ELEVATION OF 14,000 FEET, APPROXIMATELY 125 SQUARE MILES ARE PHOTOGRAPHED IN A SINGLE EXPOSURE.

chart is the land area with its characteristic shore forms, its landmarks, its elevations and depressions. In piloting, a navigator relies a great deal on prominent shore objects to fix his position, and sometimes even uses the configuration of the shoreline for identification. For mapping from the ground the planetable is still the most satisfactory instrument (Fig. 8). The introduction of the "telemeter" around 1865 greatly facilitated the surveying of complex shorelines and gave the chart increased fidelity. Ground topographic methods are rapidly giving way to the more economical and more expeditious method of aerial topography (Fig. 9). The wealth of information and fullness of detail embraced in an air photographic survey cannot be matched by any other practicable method of surveying.

Hydrographic Advances. From the standpoint of the nautical chart the greatest advances made, since the publication of the first chart by the Coast Survey, have been in the field of hydrographic surveying. The correct charting of the water area is of supreme importance to the navigator because, unlike the land area, it involves fea-

tures hidden from his view. Modern hydrographic methods permit the accurate delineation of the ocean floor with its intricate patterns of submerged valleys, shoals, ridges, and plateaus. These characteristic features serve the navigator as permanent submarine "landmarks" for identifying his position at all times, irrespective of adverse weather conditions.

Hydrographic surveying consists essentially of the measurement of depth and the determination of the survey vessel's position at the time the depth is obtained—both of which are necessary in charting. The revolutionary advances made in this field during the past two decades have had a profound effect on the usefulness of the nautical chart. The old and well-known methods of sounding—the hand-lead and line for shoal water and the wire sounding machine for deep water—have given way to echo sounding. Echo sounding is an outgrowth of the experimental work begun in 1912 for detecting the presence and nearness of icebergs, and of the submarine detecting devices used during World War I.

Echo sounding is a method of determining the depth of water under a survey vessel by measuring the time required for a sound wave to reach the bottom of the ocean and return as an echo. We are all familiar with echoes in air and realize that they are caused by reflections of sound from some distant object. If the time is measured between the production of a sound and the reception of its echo, the distance may be determined by multiplying one-half the time interval by the velocity of sound in air.

The same principles are applied in measuring water depths by echo sounding. A sound is produced at the survey vessel near the surface of the water; it travels to the bottom, from which it is reflected back to the vessel as an echo. From the elapsed time and a knowledge of the velocity of sound in sea water the depth can be computed. Echo-sounding equipment is designed to produce the sound, receive the echo, measure the intervening time interval, convert the time interval into depths, and register these depths on a graduated dial (Fig. 10). As many as 20 soundings per second can be obtained in

Courtesy New York Daily Mirror

FIG. 10. THE DORSEY FATHOMETER—A VISUAL-TYPE DEPTH RECORDER

2 FATHOMS OR 2000 FATHOMS ARE READ WITH EQUAL FACILITY ON THE DIALS OF THIS PRECISION DEPTH INDICATOR.

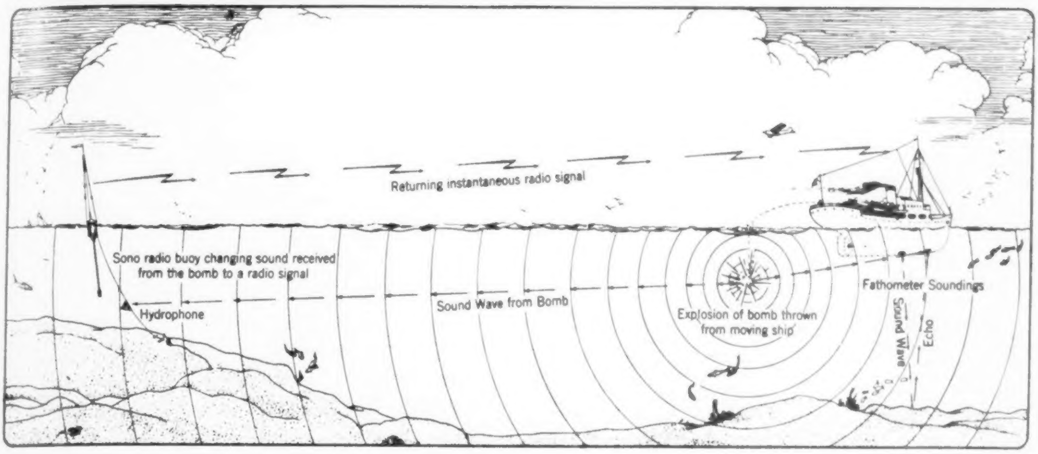


FIG. 11. MEASURING DEPTHS AND DISTANCES BY SOUND

TWO OF THE SCIENTIFIC METHODS USED BY THE COAST AND GEODETIC SURVEY TO DETERMINE WATER DEPTHS AND THE PRECISE POSITIONS OF SUCH DEPTHS ON NAUTICAL CHARTS—ECHO SOUNDING AND RADIO ACOUSTIC RANGING. BOTH OPERATIONS ARE CARRIED ON SIMULTANEOUSLY BY DAY OR NIGHT WHILE THE SHIP IS UNDERWAY.

the shoaler depths, giving an almost continuous profile of the bottom.

Our knowledge of the ocean floor has been greatly augmented by the advent of echo sounding. The measurement of great depths, which formerly required an hour or more, can now be obtained in a matter of seconds, thus making it possible to take thousands of soundings in areas where formerly only a few scattered ones were economically feasible.

Geographic position, no less than depth, is an essential element of every hydrographic survey. The early hydrographers located their soundings by sextant angles on shore objects. When out of sight of land, positions were determined by celestial observations or by "dead reckoning." Although suitable for navigation, these methods had certain inherent weaknesses when used for surveying purposes, and an accurately coordinated offshore hydrographic survey was the exception rather than the rule. These difficulties, long an obstacle to adequate charting, have been overcome by the use of under-water sound ranging and other methods (Fig. 11). By exploding a bomb in the water near the survey vessel and measuring the time of arrival of the sound wave at two or more previously located sono-radio buoys (Fig. 12), the distance of the vessel from the buoys can be determined, for the velocity of sound

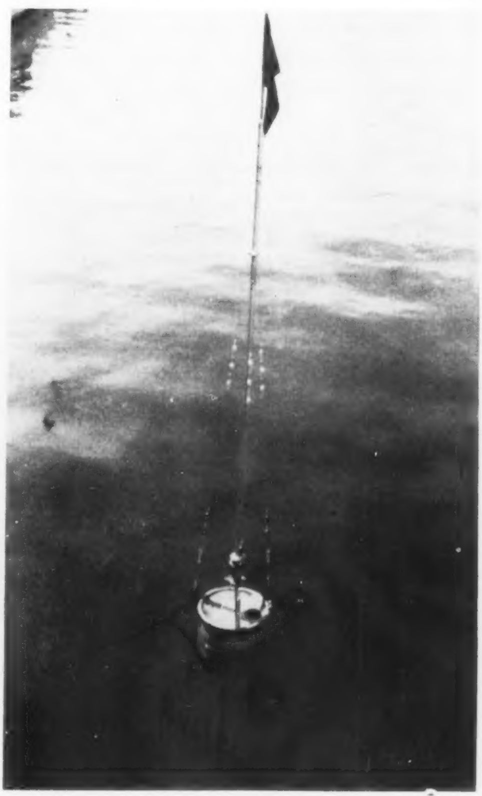


FIG. 12. SONO-RADIO BUOY

IN USE IN OFFSHORE HYDROGRAPHIC SURVEYING FOR POSITION LOCATION. THE BUOY CONTAINS THE MECHANISM FOR RECEIVING THE SOUND IMPULSE AND FOR AUTOMATICALLY RETURNING A RADIO SIGNAL TO THE SHIP.

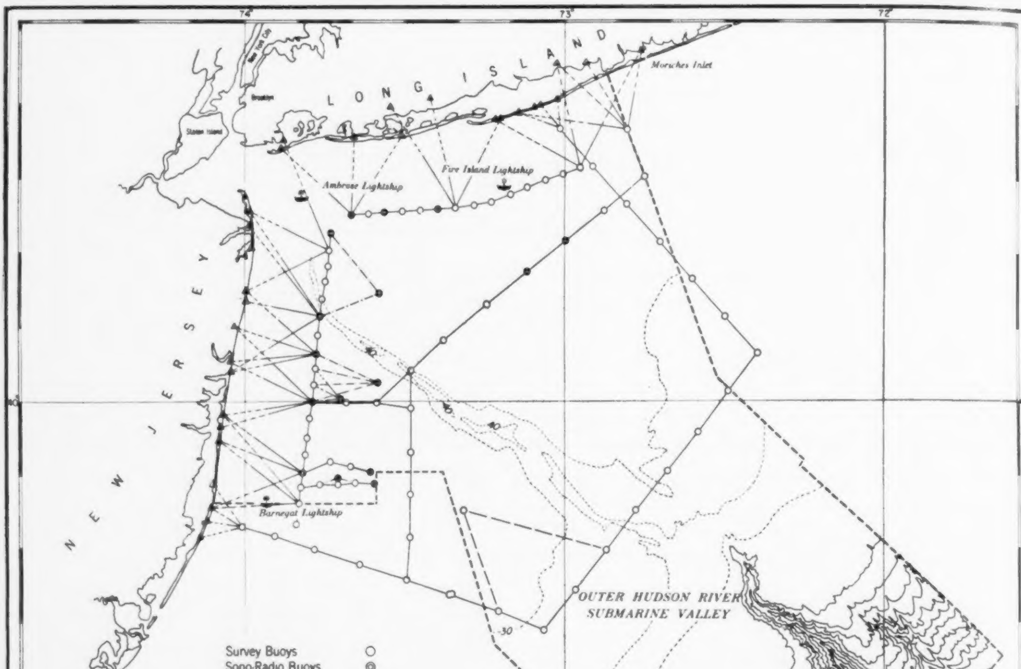


FIG. 13. COORDINATION OBTAINABLE IN MODERN OFFSHORE SURVEYS
METHODS USED TO EXTEND CONTROL FROM THE COASTLINE TO OUTER HUDSON SUBMARINE VALLEY, 100 MILES AWAY.

in sea water is known. This method of position determination is known as Radio Acoustic Ranging and was developed in the Coast Survey after World War I.

The acoustic methods of surveying are steadily pushing seaward the frontiers of accurate hydrographic surveys, and are being used today to explore the intricate patterns of our deep coastal slopes with an accuracy and completeness undreamed of by the older methods—thus adding to the safety of life and property at sea (Fig. 13). Acoustic surveying is rapid and sensitive enough to detect and chart wrecked ships lying on the ocean bottom (Fig. 14).

The Wire Drag. Some years ago a method of hydrographic surveying was developed to supplement the ordinary methods of sounding in areas where pinnacle rocks, boulders, or coral reefs were likely to exist. This consists in towing through the water a wire, sometimes a mile or more long, supported at an adjustable depth by floats and buoys. The wire will catch on any obstruction which extends above the depth at which it is set. Many dangers to navigation have been found by this method along the rocky coasts of California, Alaska, and New England, the most spectacular of these finds being located in southeast Alaska close to the steamer lane. From a depth of 650 feet it rises shaftlike to within 17 feet of the surface. The function of a hydrographic survey is to find such hidden dangers; the function of the chart is to show them accurately. Once the obstruc-

tions are charted, the navigator is put on notice of their existence and can shape his course to avoid them.

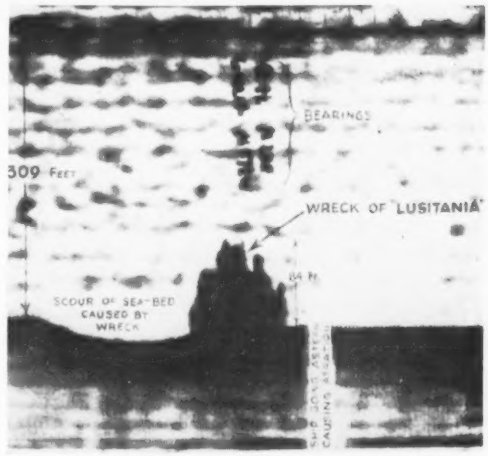
The wire-drag method is still in use today. During the war it has been instrumental in the location and charting of the many wrecks along our Atlantic Seaboard, resulting from enemy submarine warfare.

Scientific Chart Making. The improvements in navigational and surveying techniques of the past two decades have not been lost to the nautical chart maker. In the last few years a new type of chart has been issued which utilizes to the fullest extent the wealth of submarine detail contained in modern hydrographic surveys. By a judicious use of depth contours, characteristic features of the ocean bottom are brought into prominence, which is lacking on the conventional-type chart where soundings alone are used. Depth contours are to the nautical chart what land contours are to the topographic map. Imagine the difficulty in interpreting a topographic map if elevations alone were shown. From these charted features the navigator can not only identify his position, but he can also shape his course in advance so as to pass in close proximity to them.

This type of chart was not possible before the advent of modern hydrographic methods, because the contours could not be delineated with sufficient accuracy. Today it is standard practice for many of the Coast Survey charts.

Navigation by depth contours is not yet fully appreciated, and their use for charting the ocean bottom seems to have aroused some surprise among those accustomed to the conventional form of chart. It is curious to note, however, that contours were actually used for delineating the floor of the ocean even before their use on land. Two hundred years ago the Dutch engineer N. Cruquis used them to show the bottom of the Merwede River, and Philip Buache, a Frenchman, used them to outline the depths in the English Channel.

The office work of preparing a nautical chart for publication is carried through with the same painstaking care with which the field work is accomplished. No approximations and no economic short cuts are allowed to jeopardize its accuracy or to vitiate its usefulness. Experienced cartographic engineers verify every aspect of the field surveys before the data are applied to the charts. There is a distinction between a survey and a chart that should be kept in mind. A survey is an original record of field data—a chart is a compilation from such surveys. Compilation is a process of selection. Even the largest-scale chart would contain but a fraction of the information shown on a hydrographic survey. The chart compilation is an engineering product in which all the rules of engineering are meticulously observed. The special and frequently adverse conditions under which charts must be used on shipboard call for good judgment throughout their preparation. Names, notes, and symbols must be so placed that they can be easily and quickly read. Even the paper on which charts are printed is of importance in order that it may be suitable for plotting and subject to as little distortion as possible. The printing of charts is done in a temperature- and humidity-controlled pressroom and on paper with the proper moisture content to insure accurate registration of colors.



Courtesy Henry Hughes & Sons, Ltd.

FIG. 14. THE SUNKEN *LUSITANIA*

ECHO-SOUNDING DEVICES TRACE A PROFILE OF THE SEA BOTTOM OVER WHICH THE SURVEY SHIP IS PASSING.

The usefulness and accuracy of the chart depend not only upon the material entering into its construction but upon the critical appraisal of such material and upon the intelligence with which the essentials are por-

FIG. 15. RECORDS FOR ONE CHART
THIS ILLUSTRATION SHOWS THE 469 FIELD SURVEY SHEETS, SOUNDING AND TIDAL RECORDS, AND VOLUMES OF DATA THAT WERE REQUIRED IN THE MAKING OF A SINGLE NAUTICAL CHART BY THE U. S. C. & G. SURVEY.

trayed. "Easy reading is hard writing" can well apply to nautical charts. The skilled cartographer must sift from the mass of data (Fig. 15) before him the important from the unimportant, the strong from the weak, the stable from the changeable; rejecting some entirely, some in part, and coordinating and selecting from the rest the information to appear on his finished chart.

Besides these engineering elements the chart compiler must be ever conscious of the importance of artistry in the chart. There must be no crowding of matter to confuse the navigator and there must be no haphazard arrangement to throw the chart off balance. Such charts are ungainly to look at and difficult to use. The style of the lettering, the placing of the names, and the amount of hydrography and topography are factors which distinguish good work from amateurish efforts. It is sometimes erroneously thought that the more crowded the soundings are on a chart, the more thoroughly has the region been surveyed. This is true in a limited sense only, but usually it is the earmark of an unskilled cartographer.

Keeping Charts Current. The publication of a chart by no means completes the problem of the chart maker. Charts must be kept alive if they are to serve their purpose properly. They must be revised frequently to give an accurate and up-to-date picture of existing conditions. Both man and nature are constantly changing the face of the earth —breakwaters and jetties are built, channels and harbors are dredged, and new paths of commerce are opened.

The coastal region is the zone where two great physical provinces meet—the land and the sea—and where changes often take place rapidly. Wave-lifted and current-borne material has driven Rockaway Point, Long Island, a distance of over 4 miles in 100 years until a jetty built in 1934 arrested its westward growth. Rivers empty vast quantities of sediment near their mouths to build out the coastline, one of the best examples being the Mississippi. In times of storm barrier beaches are often broken through to form inlets of a temporary or permanent nature. A deep-water inlet 300 feet wide and 18 feet deep was cut through the barrier beach off the south coast of Long Island during the hurricane of September 1938.

While these visible effects on land are being produced, others even more important to the navigator are going on beneath the surface of the water. Bars and channels shift, new shoals form, old ones disappear. The safety of navigation depends upon an accurate representation of all these changes on the published charts.

Because of these changes it is necessary to print some of the charts in small issues to avoid early obsolescence. The New York Harbor Chart, for example, is printed about four times a year. It is the endeavor to furnish the navigator with the very latest information available. Sometimes the presses are even stopped to apply an important correction to the printing plate. Between printings, important corrections are applied to the chart by hand before issue.

From here on the navigator is on his own

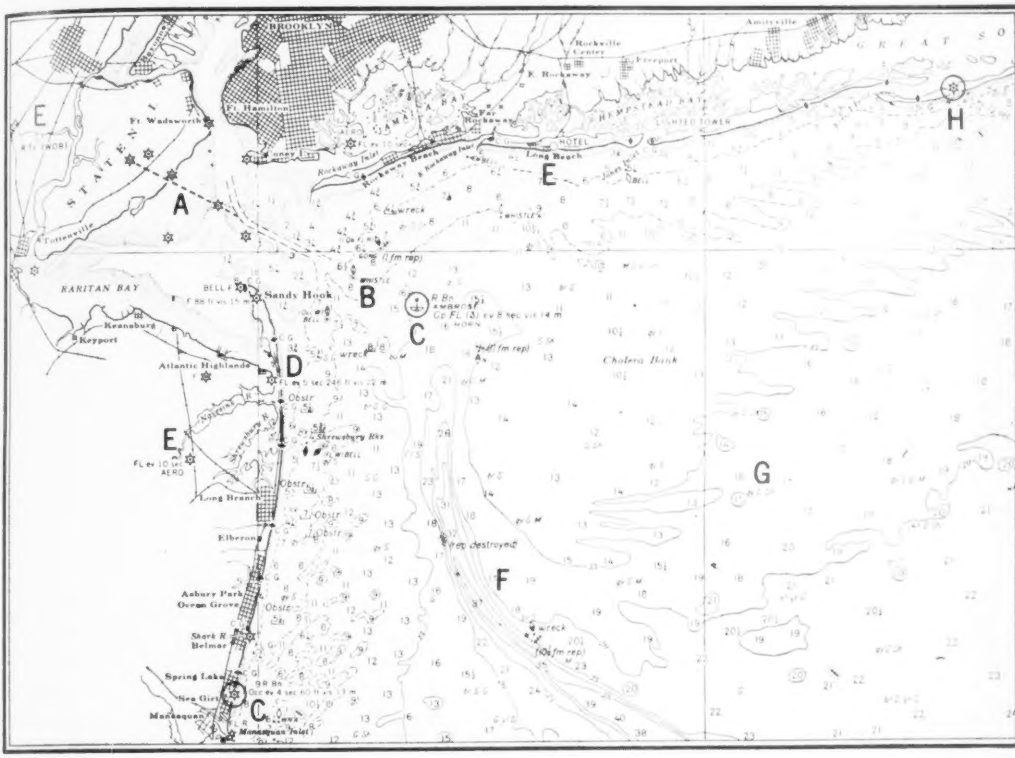


FIG. 16. MODERN U. S. C. & G. SURVEY CHART

SECTION OF CHART 1108 (REDUCED) SHOWING APPROACHES TO NEW YORK HARBOR. SEE TEXT FOR EXPLANATION.

and must bring his chart up to date by scanning the weekly *Notices to Mariners* and other special publications issued for his benefit. The locations of outstanding dangers, newly discovered, are furnished him by radio. Every effort is thus being made by the Government to keep the navigator fully informed of vital changes in the chart.

Aids for Safe Navigation. Figure 16 shows a section of the modern chart of the approaches to New York Harbor. It is of interest to note the various navigational aids the mariner has for determining his position as he leaves or approaches the metropolis:

At *A*, he has a range for a line of position through Ambrose Channel.

At *B*, he has danger warnings and channel markers—buoys by day and lights of various characteristics by night.

At *C*, he has radio beacons on the lightship and on shore for obtaining bearings by radio compass from the vessel.

At *D*, he has the height and visibility of a light for determining position.

At points *E*, he has landmarks for taking angles and bearings—structures and natural objects by day and lights by night.

At *F*, he has depth contours for use with an echo sounder.

At *G*, he has bottom characteristics, an ancient method, but still in use, for determining approximate position.

And at *H*, he has a Radio Direction-Finder Station, which furnishes him with true bearings for plotting on his chart.

One rightly wonders how the ancient mariner, without any of the modern navigational aids and contrivances, ever managed to reach his port of destination. Perhaps the answer is that very often he didn't!

The accurate chart of today is a scientific achievement. Its evolution has kept pace with the economic development of the nations and with the progress in science and engineering. For the navigator it is robbing the sea of its hidden perils; for the scientist it is giving new orientation to his physiographic concepts. And although we have moved far from its early crude form, it is

not to be supposed that the nautical chart has reached its limit of usefulness or that its character has been fixed.

While it would be difficult to predict what the full impact of the war will be on the nautical chart of the future, it is reasonable to expect that some of the techniques developed in this and related fields will affect both the character and the production of nautical charts. The period following World War I saw tremendous forward strides in the science of hydrographic surveying, with resultant effect on the nautical chart. It may well be that the adaptation of radar to marine navigation—which would make it possible to see through fog and darkness and observe the positions of ships, buoys, and shorelines—may require further modification in the present character of the chart. It is the function of a modern chart-making agency to serve the maritime public in the best and most expeditious way possible. To this end the Coast and Geodetic Survey will be ever on the alert to adapt its product to the changing conditions in the fields of marine surveying and navigation.

A. L. SHALOWITZ

A. L. Shalowitz, Research Cartographic Engineer in the U. S. Coast and Geodetic Survey, was born in Lithuania in 1892. He was graduated from the Baltimore Polytechnic Institute in 1911. In 1916 he entered the field service of the Coast Survey as a hydrographic and geodetic engineer and made surveys in the States, Alaska, and the Virgin Islands. Seeking a less nomadic life, he transferred to the Washington Office in 1921. He received his LL.B. in 1926 (with first honors) from Georgetown University and his LL.M. in 1930 from George Washington University. He has specialized in the interpretation of the surveys and charts of the Bureau, for legal and other purposes, and is the author of a number of technical papers and reports, among them *The Geographic Datums of the U. S. Coast and Geodetic Survey*, *Our Changing Coastline*, and *Navigability—A New Supreme Court Interpretation*. He has collaborated recently in the preparation of the *Hydrographic Manual* of the Coast Survey. Mr. Shalowitz is the editor of *Surveying and Mapping*, journal of the American Congress on Surveying and Mapping. His editorial and cartographic experience was evident in his meticulous preparation of the manuscript and illustrations of his present article. Nothing was overlooked that would help the editor do his best for the author and reader.

MAN'S UNINVITED FELLOW TRAVELER—THE COCKROACH*

By JAMES A. G. REHN

CURATOR OF INSECTS, THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

THE cockroaches are insects which to the average person are house-haunting pests, living secretive lives away from the light of day, and creeping into one's larder when given the slightest opportunity. Most definitely they produce in the majority of people a strong feeling of aversion. It often takes some effort to convince the "doubting Thomases" that the number of species of cockroaches which are domiciliary pests is greatly limited—in fact less than one percent of the known forms—and that cockroaches of many kinds are diurnal, with hundreds of species tropical forest foliage forms, others semiaquatic, some in one sex living in the ground, a few wood-boring, while a dozen or so genera will be found, in a state of either known or suspected commensalism, in the nests of ants, wasps or termites.

From all these far more interesting biological associations, however, we are almost always brought back to those domiciliary cockroaches which to most people give the group a reason for its existence, and it is on these that the oft-repeated questions are centered. An inevitable one is, "Where did this species come from originally?" Rather helplessly most entomologists then pick up a few standard and rather well-thumbed textbooks, and read this or that wording of stereotyped traditional explanations that this species "came from the Orient," and the other "is a native of tropical America." Unfortunately hardly any standard work of reference has given correctly the most probable original homes of some half-dozen of our better-known domiciliary, or habitation litter inhabiting, species, and almost every new treatment of these species simply repeats the erroneous assumptions of the past. The chief justification for this course will be found in the unfortunate technical specific names applied to them long ago by systematic pioneers, appellations such as *orientalis*, *germanica*, *americana*, *australasiae*, *surinamensis* and *maderae*. According to their light of many decades past these early scholars applied specific names suggested by the territory from which they received their specimens, or that from which it was believed they had come. The difficulty was that in the former case the species often had gone there as a fellow traveler with the early voyagers, and thus became one of the "first settlers."

During the past thirty-five years, individually or with my colleague Mr. Morgan Hebard, I have made a number of critical studies on the systematics and distribution of the cockroaches of both the New and Old Worlds. Mr. Hebard personally has added to these many equally important contributions, and a large amount of as yet unpublished critical information has also been assembled from the unsurpassed collections of the family which have been assembled at the Academy of Natural Sciences of Philadelphia. In addition the series of these insects in virtually all other important American institutions have been drawn upon, and we also have been able to study extensive representations from the collections of great European institutions such as the British Museum (Natural History), the French National Museum of Natural History, the Museum of the Belgian Congo, and a number of other museums scattered over the world.

In the cockroaches, or Blattidae, as in most other groups of the Orthoptera, we find a very marked degree of geographic limitation of genera, very few except those spread by commerce being world-wide in distribution. However, in analyzing problems of blattid distribution or centers of origin, it

* Read before the Entomological Society of America at its meeting in Philadelphia, Dec. 28, 1940.

is necessary to realize that we are dealing with a group which in considerable part possesses a broad range of coverage in adaptability, and also in ease of transport. We are also concerned with a very ancient group, with long-tested and often quite flexible survival powers, as the blattids would not have covered their great span of geological time if this were not so. The group has, and doubtless has had, many forms so highly specialized that they are virtually incapable of utilizing various means of transport, or of surviving in reasonably different environments if they should be so placed. On the other hand, we have a more limited number of adaptable types, which readily can be transported, and, given their required temperature and humidity tolerance, are thoroughly capable of establishing themselves fully and enduringly on the other side of the world, or at any suitable intermediate station.

Until the last forty years, our knowledge of the taxonomic relationship of many of the blattids was very unsatisfactory, particularly the concepts of generic units. While the classification of the superfamily Blattoidea remains in a somewhat unsettled condition, without general agreement as to the limits of the higher groups, in the main it is definitely on a sounder basis than it was in 1900, and our understanding of the character, limits, and relationship of many of the genera now rests upon a far greater knowledge than was possessed at that time. In consequence, it is possible by drawing upon information now available on the distribution of near relatives of species which have acquired a domiciliary status to secure important evidence as to the original homes of the latter.

We must bear in mind that domiciliary habits are acquired ones, like domesticity in the dog, cat, horse, ox, sheep, or goat. Probably some cockroaches, in a feral state, fed upon and lived in material which early men pre-empted as a food, and their passage into his habitations, in that way, was at first a physical transfer. With an assured abundance of food, particularly of varied character such as would appeal to insects which are largely omnivorous, it is not difficult to appreciate the beginning of the domiciliary habit.

Dependence of cockroaches upon human habitations, however, varies in degree, and some species, such as *Pycnoscelus surinamensis,* though occurring commonly in the litter and topsoil about buildings, are not as a rule found within buildings except greenhouses and similar structures. Again, many species are accidentally transported from tropical regions in fruit and similar products to much colder regions, and are there unable to survive, or at least propagate, in houses or similar structures, even when the proper food is provided, unless the required temperature is maintained. One species will require only a temperature above a specific minimum; for another, a certain range of relative humidity is as important as temperature.

TAXONOMIC and distributional studies of recent cockroaches, or the family Blattidae, with which I have been engaged for a number of years, have brought together a very considerable amount of information on the thousands of existing species of this ancient group. One of these is the cockroach probably best known to American entomologists and perhaps laymen as well: *Blatta orientalis,* the so-called Oriental Cockroach, Shad Roach, or Black Beetle (Fig. 1). With us it is one of our commonest household pests, at home in almost any situation which promises food and warmth. In the literature of the past we find the very general assumption that *Blatta orientalis,* the "Oriental Cockroach," came from what is rather vaguely defined as "the East." Linnaeus, the great father of systematics, in 1758 considered the species as native to America and as introduced in the East. He noted it as common in Russia and as having reached Stockholm in 1739. The exact reason for his indication of America as its original home is not at all clear, but certainly he was wrong. No members of the genus to which *orientalis* belongs or of any very closely related genera are American endemics. Carl Brunner, the great Austrian orthopterist, in 1865 felt that *orientalis* originated in Asia, and he then stated it "abounds in the East Indies as

well as in Asia Minor," but he added "it is rare on the coasts of the Mediterranean and seems to be completely absent from Greece." He then continued, "It is equally rare in Italy and in southern Spain," but noted that he had it from Algeria. In 1882 the same author, in his *Prodrome of European Orthoptera,* said merely that the species is not known in a wild state, and that in the last two hundred years it entered Europe from Asia. Miall and Denny in *The Cockroach* were more specific as to their understanding of the species, and said it "is native to tropical Asia and long ago made its way by the old trade routes to the Mediterranean countries," but why they so definitely fix its original home is not stated.

FIG. 1. ORIENTAL COCKROACH[1]

[1] Many entomologists and most pest control operators prefer the abbreviation "roach" for this and other specimens of domiciliary cockroaches. Oriental and German roaches have been seen by most citizens of the United States, who think that one is a roach and the other a water bug. It should be understood that "roach" and "water bug" are synonymous and that it would be desirable to discard the latter name.

In this and the other ten illustrations the insects are shown in their natural, or actual, size; thus, the sizes of the adults of the different species can be compared. Appropriately, the American roach is altogether superior to the Oriental and German roaches in size, appearance, and ability to use its wings in flight.

In most of the illustrations, two specimens are shown: male and female adults. In some figures the wings of a specimen are spread. In Figure 6 an immature form is also shown. For identification one of the following symbols is printed under each specimen: ♂ = adult male; ♀ = adult female, and X = immature form.—ED.

The passage of *Blatta orientalis* westward across Europe, like that of *Blattella germanica,* is well documented and need not be dwelt upon here. It is sufficient to know that into northern and north-central Europe this species quite definitely came from the East, and according to Lucas, in his *Monograph of British Orthoptera,* it had made its way to Holland and England by the time of Elizabeth. Early in the seventeenth century Swammerdam knew it in Holland, and in 1624 Moufet mentions it as occurring in wine cellars in England. There, however, its spread was much slower than on continental Europe, as Gilbert White, in 1790, speaks of it then as an unusual insect at Selbourne. As to the localization of its distribution in Mediterranean Europe to which Brunner referred in 1865, paucity of information at that time was probably responsible. Ignacio Bolívar, the distinguished Spanish orthopterist, in his 1898 *Synopsis of the Iberian Orthoptera* speaks of the species as "acclimated in the great part of Europe," without qualifying comment.

The most interesting and comprehensive summary to appear in recent years of the information then available on the native home of *Blatta orientalis* is contained in the posthumous work of Robert Shelford entitled *A Naturalist in Borneo.* For some years before his death in 1912 Shelford was our most promising student of the cockroaches, and in this interesting volume will be found many observations and conclusions assembled as a result of his residence at Kuching, where he served as Curator of the Sarawak Museum. He wrote:

> [*Blatta orientalis*] has not been met with in a truly wild state until quite recently; the first specimens that were found were caught in houses, and though it has always been assumed that it was imported into Europe from the East, I am not aware that it has ever been found in Asia except as an unwelcome guest in human habitations. The discovery (by von Adelung) of specimens in the Crimean peninsula living under dead leaves, vegetable detritus and stones, in woods and copses far from any human habitation, is a fact of considerable interest, and it is perhaps permissible now to regard Southern Russia as the centre whence this ubiquitous insect has spread.

My personal interpretation of von Adelung's find is that the original home of the species was not located, but instead a "way station" on an ancient line of travel, where the species doubtless has been established for centuries. My reason for so believing is that no wild species related to *Blatta orientalis* is known from Europe.

Taxonomic studies of the Blattidae of Africa, with which I have been engaged for a number of years, have brought to light several previously undescribed, wild, close relatives of *Blatta orientalis;* one from Uganda, another from Kenya Colony. Like these species, all the other wild forms properly referable to *Blatta* as now restricted—which means all except *B. orientalis*—are African, ranging southward to the Cape of Good Hope and west to the Cameroons. Examinations of large series of as yet unreported Oriental Blattidae disclose no specimens of species of *Blatta,* and the literature, as mentioned by Shelford, gives no concrete information on the occurrence of *orientalis* there, except in a few large ports serving a world commerce. All indications are that *orientalis* does not thrive in the true lowland tropics, and the only parts of South America where the species seems to have been well established for many years are Chile and Argentina, neither of which is truly tropical. Philippi recorded *orientalis* (under a synonymic name) from the former country as early as 1863. Other early American occurrence records were from Jamaica in 1842 by Sell, from Guadeloupe in 1837 by Lherminier, and from Honduras in 1868 by Walker. There has been nowhere in tropical America as complete occupation of a country by *orientalis* as in the U. S.

Turning back to Africa, the records of *orientalis* show that, except for its presence at Windhoek in Southwest Africa, at Cape Town, and in Natal—these clearly detached colonies established by commerce—it came from Morocco, Algeria, Tunis, Tripoli, Cyrenaica, Egypt, and Somaliland. Finot has reported the species in Tunis as occurring in desert encampments. The North African material which I have examined is from localities reaching from Mogador, Morocco, to the Sinai Peninsula.

From the positive and negative evidence now available, I feel justified in concluding that *Blatta orientalis* was originally a native of North Africa, and that it probably found its way into eastern Europe in Greek, or even Phoenician, vessels, spreading into Byzantium, Asia Minor, and the Black Sea region, and thence slowly northward and westward over the remainder of Europe. The colonies in Chile and Argentina were doubtless established by way of Spain; where it probably was introduced from Moorish lands in North Africa long before the species, in its westward spread over most of Europe, had reached adjacent France. Over most of the continent of North America *orientalis* is as much at home as in Europe, but in the more humid southern United States it yields its usual role quite generally to the species of *Periplaneta.* Similarly in the humid tropical areas of South America *orientalis* has made little headway, and there it is not the domiciliary problem which the Periplanetas are. In brief, *orientalis* seems to have been derived from an area which combines summer heat and moderate winter cold, as the species can stand more of the latter than the Periplanetas, yet is not adapted to conditions of tropical or subtropical humidity. I have little doubt that comprehensive work in North Africa will disclose *Blatta orientalis* living under the same conditions of freedom from dependence upon human habitations as noted in the Crimea. Certainly the nearest known relatives of *orientalis* are wild forms of east-central Africa.

PROBABLY the most ubiquitous species of cockroach, and one certainly as well known as the Oriental Cockroach, is the so-called German Cockroach, or Croton Bug, (*Blattella germanica*) (Fig. 2). There are many other names for it; the English call it Shiner or Steam Fly; in Russia it has been called the "Prussian," and in Prussia it was known as the "Russian." Its steady spread across Europe was very similar to, but definitely more recent than, that of the Oriental Cockroach. In England it seems to have become broadly established only by the middle of the last century, and, according to

an anonymous writer quoted by Miall and Denny, was supposed to have become established at Leeds by means of bread baskets of soldiers returning from the Crimean War. Burr, writing in 1936, says it had been established in England for a century. Brunner in 1882 quoted Fischer de Waldheim to the effect that the species occurred feral at Moscow, and that it was similarly present in Thuringia, Saxony, the Hartz Mountains, in Westphalia, and at Kloster Neuburg near Vienna. Brunner then added, however, that he had never found it in a wild state. In 1898 Ignacio Bolívar stated that it was encountered in all of the Iberian peninsula and the remainder of the Mediterranean littoral.

As with *Blatta orientalis*, most authors give the original home of *germanica* as "Asia," and consider that it reached western Europe across Russia and Germany.

♂ ♀

FIG. 2. GERMAN COCKROACH

The very imperfect appreciation, until recent years, of generic limits in the complex group or genera to which this species belongs makes any deductions on the basis of generic distributions as of the literature prior to 1910 virtually valueless, as *germanica*, before that time, was considered a member of an enormous, entirely unnatural "blanket" genus, now more logically broken into a considerable number of components and even into several genera groups. Thus the evidence of closely related species as indicators of the probable original home of *germanica* is our sole reasoning point. Purely historical information, of course, is not available, except as to its spread in the last century or so.

We have in the Oriental region a limited number of species now generically associated with *germanica*, but all of these show very definite morphological differences, and none is from what is generally assumed to be the "Asia" of ordinary language; that is, Central Asia. Instead they are Indo-Iranian and chiefly Indian in distribution. On the other hand, in northeastern Africa, from between the great African lakes and Eritrea and the Anglo-Egyptian Sudan, occur fifteen distinct species intimately related to *B. germanica*, which also occurs there as well as westward across northern Africa, much as does *Blatta orientalis*, although *germanica* is also of broader establishment at many localities in the most tropical parts of the continent.

Therefore it would seem that the early human associated history of *Blattella germanica* is essentially the same as that of *Blatta orientalis*: From northeastern Africa it was transported by Greek or Phoenician vessels to Byzantium, Asia Minor, and the region of the Black Sea. In much of southern Russia it remained for centuries until the gradual opening up of occidental commerce with that country, probably after the Thirty Years War, made the passage of the species westward a possibility. It then spread gradually over western Europe and thence to America, and by commerce to virtually all parts of the world. The tolerance by this species of many conditions apparently not acceptable to *Blatta orientalis* has made its distribution much more cosmopolitan than that of the larger species.

THE cockroach genus *Periplaneta* is made up of a number of fully winged and active species, of which at least three have become domiciliary in habits, and two of these are outstanding pests in tropical, subtropical, and even warm temperate areas of virtually the entire world. These are the so-called American Cockroach (*Periplaneta americana*) (Fig. 3) and the equally poorly named Australian Cockroach (*Periplaneta australasiae*) (Fig. 4). In the United States the first of these is found quite generally as a domiciliary insect over most of the warmer, more southern area, frequently taken as far north as New England, but to the northward only under definitely protected conditions

The Australian Cockroach is more partial to consistently warmer conditions and can exist continuously over much of the United States only under conditions of maintained warmth.

Most of our older, and some modern, authorities assumed that Linnaeus and Fabricius respectively were correct in the implications of the names they gave to these

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FIG. 3. AMERICAN COCKROACH

species; i.e., that the first originated in America and the second in Australia. It is yet asserted regularly, and with rather monotonous unanimity, that *americana* is native to tropical America and that *australasiae* came from the Antipodes. However, the far-seeing Shelford questioned this in his *Naturalist in Borneo,* saying, "it is certain that *australasiae* is only a rare immigrant to Australia, and I believe that tropical Africa or perhaps South-Eastern Asia was its original home."

No nondomiciliary species of *Periplaneta* occurs in the New World, except for the localized occurrence in our southeastern states of one species clearly introduced, which can be found in a variety of situations, as in houses, about buildings, under signs or on wharves, but always within the limits of cities or towns. The exact origin of this species is still uncertain, and it is as yet unknown from the nearby West Indies. Native nondomiciliary species of *Periplaneta* occur in many parts of tropical and southern Africa and in the Indo-Malayan region. The nearest relative of *Periplaneta* is *Pseudoderopeltis,* which is a dominant and peculiarly African genus with a score or more species, occurring from Senegal and Egypt to the Cape. Throughout tropical Africa both *Periplaneta americana* and *australasiae* occur almost everywhere under domiciliary conditions, and in the vicinity of, as well as in, buildings, huts, and shelters of all kinds. Both are now very abundant in tropical America under domiciliary conditions, but there they are not as frequently encountered outside of human structures as in tropical Africa, as I can testify from personal experience across the width of Central Africa, in the West Indies, as well as in a number of countries of Central America and several of South America. Apparently, the occasional European records of these two species have been due to individual commercial introductions and not to sheet infiltration as in certain other species.

From our present knowledge, I feel we are warranted in concluding that, though *Periplaneta* also occurs native in Indo-

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FIG. 4. AUSTRALIAN COCKROACH

Malaysia as well as in Africa, the evidence points more directly to tropical Africa as the original home of *Periplaneta americana* and *australasiae,* and perhaps the less frequent *P. brunnea* as well. Slave ships from the West African coast, continuously moving for nearly two centuries, doubtless provided the means of introduction into South America, the West Indies, and the southern United States. The flying ability of *Peri-*

planeta, which is often exercised and by both sexes, has furthered the broadening of distribution when colonies had been established. The sole controlling factor with these species seems to be the maintenance of temperature above a certain minimum. Along the periphery of their distribution they can survive only under protection in greenhouses and similar uniformly heated places.

Perhaps the most pleasingly patterned of our domiciliary cockroaches is *Supella supellectilium* (Fig. 5), for which the vernacular name Brown-banded Cockroach is the most appropriate of several which have been used. First described by Serville from Mauritius, it was, as its specific name indicates, recognized by him as a household form. The species is now known from a considerable part of the tropics and subtropics of the Old World, although apparently much less evenly or broadly distributed to the eastward than it is over eastern, southern, and northeastern Africa. It was introduced into the West Indies probably by slave ships and first recorded there in 1862 by Saussure, the great Swiss orthopterist, as the synonymous *Blatta cubensis.* A few records are available of its occurrence at coastal points in South America, but it is not at all broadly established there or in Central America. From the United States I first reported it in 1903 as taken at Miami, Florida; doubtless introduced from Cuba, where it is quite abundant in houses. In 1912, with my colleague Mr. Morgan Hebard, I found it common in a fruit store in Key West, and in recent years its distribution in the United States has been steadily extended, so that today it is known to occur under domiciliary conditions as far northeast as Philadelphia, westward to San Bernardino, California, and in the interior northward to Nebraska. Shipments of fruit from Florida have probably provided a ready means for distribution in our territory.

The genus *Supella* is now under critical taxonomic study, and there are yet to be properly characterized a number of native African species living under natural conditions and not dependent upon human habitations. The species *supellectilium* is distributed over much of Africa outside of the Guinea forest areas. No nondomiciliary species of *Supella* is known except from Africa, and it is therefore quite reasonable to conclude that the genus, hence the species *supellectilium,* is of African origin, and that slave ships probably were responsible for its American introduction.

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FIG. 5. BROWN-BANDED COCKROACH

The most widely distributed member of the blattid subfamily Panchlorinae is a species which possesses no accepted vernacular name, but which may be called the Bicolored Cockroach (*Pycnoscelus surinamensis*) (Fig. 6). Supposed by Linnaeus to be of American origin, he gave to it the specific name *surinamensis.* While the subfamily Panchlorinae has many endemic American species and a considerable number of genera so limited, *Pycnoscelus* is clearly not one of these. The species *surinamensis* is virtually world-wide in distribution within the humid tropics and subtropics and is less likely to occur within doors than under stones, boards, tiles, dead palm trees, or any other loose litter or trash about houses or stables. It has also been taken from under the bases of living palm leaves, in bromeliads, under boulders away from houses, in rotted logs, in cracks of semidried mud and in the litter of wood-rats' nests. The very different-looking immature stages are often found burrowing in topsoil. Within the United States

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FIG. 6. BICOLORED COCKROACH

surinamensis has been reported as established outdoors in peninsular Florida, in Louisiana, and in southern and south-central Texas. In greenhouses and similar places with artificial heat during cold weather it may occur as far north as New England.

This widely spread *Pycnoscelus* has one peculiarity which may have a confirmatory value in establishing its original home. Virtually everywhere in the Western Hemisphere, and probably in other areas in which it occurs, the species is apparently parthenogenetic. As yet I do not know of an adult male specimen taken under condition of nature in the New World, the single case brought directly to my attention being a male captured in the greenhouses of the New York Botanical Garden, the source medium for which may have been a recent importation of Oriental shrubs. As far back as 1865 Carl Brunner noted in the series of *surinamensis* before him that the only males were from the East Indies, none being included in his tropical American representation. In 1893 the same author said he had seen additional males from Burma, but still none from the New World. Hebard in 1916, examining much larger series than Brunner had seen and from a considerable number of localities, noted only one male from Lombok in the Lesser Sunda Islands. In 1927 Rehn and Hebard found no males among fifty adult specimens from twenty-four West Indian localities. Since that time series totaling many hundreds of specimens from a very large number of localities have been examined by me. Males were found only in Oriental representations.

In Indo-Malaysia occur other endemic species of the genus *Pycnoscelus*, none of which ranges over more than that general area. This, taken with the inference which can be drawn from the localization of males of *P. surinamensis*, leads one to the conclusion that this species is of Oriental origin, and that it owes its present wide distribution to commercial transport, augmented by its habit of hiding in soil, thus making possible its transfer with plant stock earth. Its introduction into Africa may have been due to Arab traders, who for a considerable time before the Portuguese reached the East coast of Africa had carried on an extensive commerce across the Indian Ocean to the east. Again, its introduction there may have been due to the Portuguese voyageurs themselves. In western Mexico it may have been introduced in Spanish galleons from the west, as discussed under *Neostylopyga rhombifolia*. For most of tropical America on the Atlantic side slave ships probably furnished the means of introduction, as they did with so many other species of insects brought from Africa. With accumulated litter and trash slave ship holds carried to America many undesirable immigrants. Possibly the introduction of *surinamensis* into the United States was a secondary one from the West Indies, where the species is known to be firmly established in all of the Greater Antilles and a number of the Lesser group.

In 1926 Fielding showed that in Australia *Pycnoscelus surinamensis* is the intermediate host and agent for the transmission of chicken eye worm (*Oxyspirura parvovum*). The parasite was found to be present in both the abdominal and thoracic cavities, as well as in the legs of the cockroach, and passed to the fowl almost immediately after *Pycnoscelus* reached the bird's crop.

A LARGER and quite striking member of the subfamily Panchlorinae is the so-called Madeira Cockroach (*Leucophaea maderae*) (Fig. 7), which is broadly established in the West Indies and in coastal Brazil, with more

recent and localized colonizations in Central America, but in the United States has as yet been taken only as an adventive brought in on bananas or similar shipments. In all probability it eventually will become established in our Southern States, as it is universally prevalent in Cuba, Jamaica, Hispaniola, Puerto Rico, and the Bahamas, where

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FIG. 7. MADEIRA COCKROACH

it frequents habitations, warehouses, and other structures. At times it is a very abundant and serious pest.

Palisot de Beauvois first reported this insect from America in the early years of the nineteenth century, presumably from Hispaniola. He then stated his belief that it originated in Africa, and that it was imported into the French colonies in America. In all the Greater Antilles and a number of the Lesser ones, as well as in the Bahamas and the Virgin group, the species is now thoroughly established. Outside of tropical America and tropical Africa *maderae* is also known from Madeira, the Canaries, Morocco, Andalusia in Spain, and Corsica, doubtless as infiltrations in colonial commerce with West Africa. In Asia and the Pacific Islands it is known only from Java, the Philippines, and the Hawaiian group. Its presence in Java and the Philippines can be explained by accidental colonial introduction from Africa, either directly or secondarily from the Canaries or the western Mediterranean region, and in Hawaii by more recent transplanting, probably from the Philippines. The absence of the species from India, Australia, southern China and the greater part of Malaysia attests its nonendemism there.

The other five members of the genus *Leucophaea* are entirely tropical African in distribution, and *maderae* also occurs over most of that continent south of the Sahara from Senegal to Kenya Colony and to Angola and Natal. A very closely related species is restricted to West Africa between Liberia and the Gabon. It appears very probable to me that *maderae* was originally a native of West Africa, and probably that portion usually spoken of as Upper Guinea, where it commonly occurs today and where its nearest ally (*L. puerilis*) is also found. Slave ships doubtless brought the species to the West Indies and the coast of Brazil prior to 1800, thus establishing it in the New World.

PROBABLY the most bizarrely marked domiciliary species is the Harlequin Cockroach (*Neostylopyga rhombifolia*) (Fig. 8). Both sexes of this strikingly patterned species are flightless, the tegmina, or forewings, being but short, lateral, articulate, but functionally useless, slips, while the hind wings are absent. Male individuals of *rhombifolia* ordi-

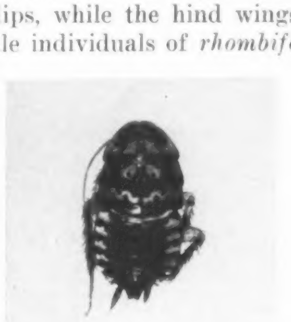

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FIG. 8. HARLEQUIN COCKROACH

narily occur much less frequently than females, but we have no evidence that males are unnecessary for reproduction as in *Pycnoscelus surinamensis*.

The first record of *rhombifolia* from the New World was of its occurrence at Acapulco, Mexico, in Venezuela, and in Argentina (Brunner 1865). In 1893 Saussure and

Zehntner reported it from Brazil. There has been little amplification of the other New World records in recent years, but the west Mexican colony has been productive of a spread of the species along the west coast of that country, northward over Sinaloa, and even to Nogales on the Sonora-Arizona line, as well as its establishment for nearly fifty years in the southern part of Lower California.

The species *rhombifolia* is abundant over the greater part of the Indo-Malayan region, particularly in the Philippines. It is also found in the Hawaiian Islands, probably as an introduction in recent years from the Philippines, and it is also quite general in Madagascar, Mauritius, Rodriguez, in the Seychelles and adjacent islands, and along the eastern coast of Africa, there extending inland along trade routes to Nyasaland and the Zambesi valley, while it has also been reported from Madeira. The last is probably an isolated colony established by a chance introduction on a Portuguese ship Europe-bound from the Indian Ocean.

What particularly interests us is the introduction of the species on the west coast of Mexico a matter of more than eighty years ago. Very probably if the species becomes established as a domiciliary insect in the United States it will be from this colony. Acapulco was the port at which the classic Spanish galleons from the Philippines landed their cargoes for land transfer to the Atlantic side, to be reloaded for Spain. Rather curiously, we have an exact parallel to the problem of *Neostylopyga rhombifolia* in western Mexico in the cases of the legless lizard, *Typhlops braminus,* and of two other reptiles, *Peropus mutilatus* and *Hemidactylus frenatus,* which, as Taylor has recently shown, were certainly introduced from the Philippines into western Mexico, and in all probability by way of the galleons reaching Acapulco. There can be little question that this now broadly spreading colony of *Neostylopyga* was an additional galleon immigrant.

The Indo-Malayan region was clearly the original home of *Neostylopyga rhombifolia,* and the occurrence of the species even on the east coast of Africa is certainly due to the inadvertent agency of man.

A PRETTY domiciliary species of much of the tropics is the Cinereous Cockroach (*Nauphoeta cinerea*) (Fig. 9), which, although not as yet found in the United States, is known from Cuba, Hispaniola, Mazatlan in Mexico, Brazil, and the Galápagos. Its introduction into the United States is quite conceivable when the breadth of its present world cover-

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FIG. 9. CINEREOUS COCKROACH

age is considered. In Indo-Malaysia it is broadly if not solidly distributed—in the Philippines, Sumatra, and Singapore—and it also occurs in Australia, while eastward it has reached New Caledonia and the Hawaiian Islands. It also occurs in Madagascar and Mauritius, and its East African records reach from Egypt, through the Sudan (where it occurs even in the huts of the Shilluk natives), to eastern Tanganyika. It has also been reported from the Transvaal and Natal, and there is one record from the Cameroons in West Africa.

In a recent detailed study of the African distribution of this and certain other African species of the genus, it is concluded that its native home was East Africa, that it spread to the Malagasy region probably through the medium of Arab trading ships, and that the more distant Philippine and similar Oriental centers were established through Portuguese or Spanish voyageurs. From the Philippines the western Mexican colony was probably founded by transport, in Spanish galleons, as with *Neostylopyga rhombifolia,* already discussed, that in Brazil by Portuguese traders on long voyages with well-established ship colonies of *cinerea,* while the Galápagos population doubtless was due

to camps of tortoise-hunting seamen from ships of numerous nationalities. The Cuban and Hispaniolan representatives may have come from the west in goods brought from the Philippines via Mexico, as the Atlantic galleons often called at Cuban or Hispaniolan ports in the sixteenth and seventeenth centuries.

An interesting case of localized introduction of a domiciliary species is that of the Buprestid Cockroach (*Oxyhaloa buprestoides*) (Fig. 10), which is a widely distributed African species, now long and thoroughly established in a localized territory in eastern Cuba. The genus otherwise is Ethiopian in its range. Most curiously the first technical name applied to this species, and that which we must use for it, was based on Cuban material, which, however, is entirely inseparable from very extensive African representations now available. Certainly the species was established in Oriente Province, Cuba, prior to 1862, but *buprestoides* has not as yet extended its range in Cuba over more than the eastern part of the island, although in tropical Africa, as I know from personal experience, it is widely distributed and abundantly represented. In 1893 Saussure and Zehntner reported the species from Mexico

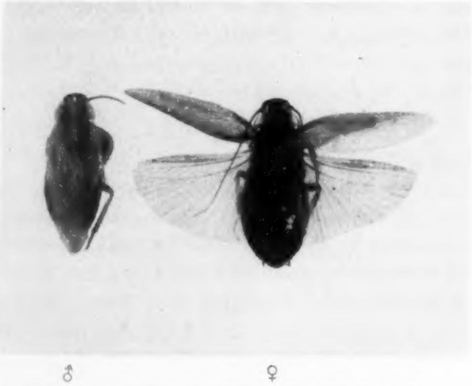

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FIG. 10. BUPRESTID COCKROACH

and Guatemala. These specimens more probably represent immigrants from the Cuban colony, possibly through the port of Santiago, than direct introduction from Africa. While *Oxyhaloa buprestoides* is widely distributed over tropical Africa, it has not been reported from the Oriental region, the American localities being the only extralimital ones, if that word may be used. I feel no hesitation in concluding that the New World occurrence of the species can be traced directly or secondarily to slave ship introduction from the West African coast.

Another quite attractive domiciliary cockroach is one for which no vernacular name has been used, but which may be called the Pale-bordered Cockroach (*Leurolestes pallidus*) (Fig. 11). It was described from and

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FIG. 11. PALE-BORDERED COCKROACH

is common in Cuba, where it is found all over the island in houses, under lockers, boards, etc. It also occurs in Jamaica, Hispaniola, Puerto Rico, and certain of the Lesser Antilles. It has been recorded from Mexico, Guatemala, and Brazil, as well as the Canary Islands and southern Florida, where it has been encountered in Key West and on Key Largo. At Key West Hebard and I found it in a fruit store associated with *Blattella germanica*, *Periplaneta americana*, and *Supella supellectilium*, which gives an idea of its ecological associates.

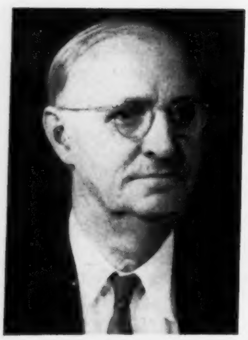
I believe the occurrence of *pallidus* in the Canaries is due to colonization from the West Indies, and that the species, and incidentally the genus, is of West Indian origin. In *Leurolestes* we have, I am convinced, a reversal of the usual flow of blattid immigration; that is, movement from instead of to the West Indies.

Of the preceding eleven domiciliary species of cockroaches, five (*Periplaneta americana*, *P. australasiae*, *Supella supellectilium*, *Leucophaea maderae*, and *Oxyhaloa buprestoides*) very definitely reached America by the slave ship route from West African

sources; another (*Nauphoeta cinerea*) is also of African origin, but in part at least reached America by a more circuitous route; one (*Neostylopyga rhombifolia*) was probably of Indo-Malayan origin, or at least came from the Indian Ocean area; two (*Blatta orientalis* and *Blattella germanica*) almost certainly reached America directly from Europe, which, however, represented a way station on the long trek of these originally north or northeast African types; one (*Pycnoscelus surinamensis*) is of Oriental origin, but probably in part at least reached America via Africa in slave ships; and one (*Leurolestes pallidus*) is an endemic West Indian type, slowly spreading by commerce into southern Florida and other parts of the American tropics and subtropics.

Another ten years of careful checking on the presence in various parts of the world of certain of these fellow-travellers of humanity may greatly amplify our knowledge of what might be called the prehistory of their wanderings, but the basic conclusions here presented represent the results of some decades of careful study, and probably will be strengthened, rather than contradicted, by information yet to be secured.

JAMES A. G. REHN



James A. G. Rehn, Curator of Insects at the Academy of Natural Sciences of Philadelphia, was born in Philadelphia, in 1881. A boyhood interest in zoology eventually crystallized into a life-time application to entomology. Appointed in 1900 a Jessup Fund Student at the Academy, his life has been spent continuously at or in the service of that historic institution, of which he also has been Secretary or Corresponding Secretary for twenty-five years. His published entomological researches upon the systematic, distributional, and phylogenetic aspects of the Dermaptera and Orthoptera, the special fields of his work, total approximately three hundred titles. In developing the largest existing collection of these insects at the Academy, a considerable portion of his life has been spent in contributory field work, involving the entire United States, portions of Central and South America and the West Indies, and a cross section of Central Africa. In 1940 he served as Secretary of the Biological Sciences Section of the Eighth American Scientific Congress. For the past few years he has been President of the American Entomological Society, of Philadelphia, the oldest and most historic of its character in this country, and is the 1945 President of the Entomological Society of America.

The foregoing was written by Mr. Rehn. It is of interest to add that his enthusiasm for field work seems to be undiminished. In reply to an inquiry Mr. Rehn's associate, E. T. Cresson, Jr., wrote as follows: "We do not know where he is or whether he will return before mid-September. The removal of gasoline restrictions has allowed him to extend his collecting itinerary."

HUMAN ENGINEERING IN THE ARMY AIR FORCES*

By Major SAMUEL R. M. REYNOLDS

RESEARCH ASSOCIATE, CARNEGIE INSTITUTION OF WASHINGTON, ON LEAVE OF ABSENCE

OUR success in making it possible for our airmen to keep pace with their aircraft has been the result of the efforts of medical men and engineers, on the one hand, and of the lessons learned in combat, on the other. The successes are the more notable because the difficulties to be overcome were great and many, while the time in which success had to be achieved was short. The intensity of V-weapon warfare on our bases in England serves as reminder of this.

The human problems to be overcome stem from the following facts:

Man in flight is different from man on the ground, resulting from the effects of lowered barometric pressure and temperature upon the body.

The speeds at which some aircraft can fly—nearly 1000 feet per second— must be controlled by a body, some of whose reflex reactions require a fifth of a second—time for the aircraft to travel about 200 feet.

The centrifugal accelerations encountered, even in heavy bombers, may transiently impose upon the body a force equal for a moment to twice that of gravity, so that a man weighs, with equipment on him, 400 pounds instead of 200 pounds. The problem of controlling voluntary movements in or about an aircraft, along a cat-walk, or by an open waist window, or to escape through a small escape hatch, is difficult.

One good look at the environment of the combat flyer will explain what I mean. Take, for instance, the operation of a B-17 Flying Fortress or a B-29 Superfort. A glance in the pilot's cabin will convince one that the flight of a 30-ton B-17 is actually a complex engineering operation, demanding a coordination of manual and mental skills which put the driving of a five-ton truck or even a streamlined locomotive in the kiddy-car class of human learning. The compartment is lined—front, sides, ceiling, and part of the floor—with controls, switches, levers, dials, and gages. I once counted 130 of them in a B-17. I have seen a B-29 having a gross weight of 128,000 pounds take-off, fly, and land as if it were a commercial air liner.

The efficient operation of all the gadgets on an airplane would be difficult in the swivel-chair comfort of an air-conditioned office. *But*—cut the size of that office to a five-foot cube, engulf it in the roar of four 1,200-horsepower engines, increase its height above ground to four or five miles, reduce the atmospheric pressure by one-half to two-thirds, lower the outside temperature to 40 or 50 degrees below zero. Then, to meet these conditions the flyer must don bulky flying clothes with suit, gloves, and boots all heated through an electric cable, strap on his parachute harness, put on a "Mae West" life preserver vest, put on a flying helmet with earphones and another wire attachment, cover his face with an oxygen mask containing a microphone, making sure that the oxygen supply hose is connected and the microphone wire is plugged in, add a heavy flak helmet and about 23 pounds of body armor.

The man is now ready to go to work, not alone, not on a single problem, but as a member of a team of 10 men working on a multitude of problems. With them he will solve, while flying from one pinpoint on the map to another some 600 to 1,500 miles away, the higher mathematical relationships of engine revolutions, manifold and fuel pressure, aerodynamics, fuel consumption, oxygen supply, barometric pressures, altitude, air speed, ground speed, wind drift, compass heading, position, and plane attitude. He and his crew will do all this whether they can see the ground or are flying through an overcast, and they will do it not as an isolated unit but in relation to their

* From an address delivered before the Society of Automotive Engineers, District of Columbia Section, Statler Hotel, Washington, D. C., on May 8, 1945.

position in a formation of several dozen other bombers.

Of course, there may be occasional individual interruptions in flight operations due to an earache from pressure changes, a joint pain from the bends, a cramp from intestinal gas expansion, a little dizziness or nausea from air-sickness, a frost-bitten hand, a general numbness from cold, or the subtle effects of oxygen-lack.

Now consider the impact of these *unearthly* forces on the body. Flight destroys the sense of security and well-being which man, as the result of thousands of years of evolutionary adaptation, derives from "keeping his feet on the ground." The spirit of adventure and the thrill of flying are so strong that they quickly overpower the natural anxiety induced by projection into a strange environment. Yet, while man learns to like the idea of flying and develops a sense of mastery in the air, he remains either consciously or subconsciously afraid of his new habitat, and when he gets into trouble in the air he becomes acutely aware of his fear of falling. This is the psychological picture in *peaceful* flight.

Interject the greater fears of combat—the enemy fighter plane closing in with guns blazing—the setting of a steady course through a seemingly impenetrable wall of flak—the sickening bump when a shell tears a hole through wing or fuselage—the sight of other planes in the formation going out of control and at times exploding in mid-air—the danger of gasoline catching fire and engulfing the plane in flame—the sitting-on-a-keg-of-dynamite feeling of carrying a bomb load—the ordeal of dead or seriously wounded crew mates on board—the danger of asphyxiation or freezing if the oxygen or electrical systems are shot out—the hazard of having to bail out and clear a spinning plane with one's parachute—the possibility of a crash landing or a ditching at sea.

In this bizarre situation of intense concentration amid intense distraction, the fear of death becomes very real.

The maintenance of bodily efficiency of the flyer in the face of these stresses and strains is a trying task of aviation physiology. It is not easy. The aeronautical engineer has carried the mechanical performance of aircraft far beyond the physiological limits of the human body; technological warfare has imposed an abnormal load on the psychological motivations of the human spirit. The problem is to take a man whose body is adapted to function in a ground environment and whose mind is conditioned to seek peace and security and then fit him for the life of a flying, fighting animal. Since we cannot build the flyer to specifications or wait for evolution to turn him into a superman, we have but two alternatives: One is to select and train the individuals best fitted physically and mentally for this duty; the other, to *provide them with devices and methods for protection against their limitations.* It is this second alternative that will concern us here, although the first is a fascinating story in its own right.

HAVING done everything practicable to pick the best qualified man for flying training, the Army Air Forces' next task is not exclusively that of making him an expert in the mechanical and tactical operation of an airplane. High altitude flight imposes physiological stresses which require exacting application on the part of the flier to maintain his efficiency. Such forces as barometric pressure or temperature are capable of injuring or killing an airman who loses control of the environmental situation at high altitude. Therefore, a vital portion of his training must concern the body in flight. This necessitates training the airman in the rudiments of physiology, so that he is aware of the dangers of high altitude and other factors in operational flying, and of the absolutely necessary means of guarding against them.

Consequently, an "Altitude Training Program" was organized in 1941 and put into operation in 1942. This grew out of study of the physiological effects of atmospheric pressure changes dating from World War I and earlier; in fact, from the time De Rozier made the first human flight in 1783. The AAF training program makes use of such standard training methods as lectures, discussions, demonstrations, motion pictures, and literature. It is distinguished, however, by its use of low-pressure, or altitude, chambers in simulated flights made by groups of trainees. There have been more than 60

such altitude chambers in 45 Altitude Training Units. The units are operated by Aviation Physiologists with the assistance of enlisted technicians. The Aviation Physiologists include some medical officers, but are mainly doctors of a biological science trained in aviation physiology at the School of Aviation Medicine. Since July 1943, trainees have made more than a million man-flights in altitude chambers. Each airman is required to make three chamber flights during the course of his flying training, one to 30,000 feet pressure altitude and two to 38,000 feet. The first flight is devoted largely to a demonstration of the effect of oxygen lack. This brings home to each flier the subtle dangers of oxygen want as the first step in convincing him that rigid oxygen discipline is necessary for survival at high altitude. The second flight emphasizes use of the standard types of aircraft oxygen equipment, and the third, usually given a short time before the flier goes overseas, deals with the practical problems of operational flying and is administered to the aircrew as a unit.

While altitude indoctrination gives major emphasis to the problem of anoxia, it also covers the effects of cold, the effects of blocking of the ear and sinus passages with changing gas volumes and pressures, the bends, intestinal gas expansion, blacking out due to centrifugal force, and the profound difference that exists between vision in daylight and in dim light, at night.

The Altitude Training Program has been closely coordinated and integrated with the work of the Personal Equipment Officer, who is, in effect, the man responsible for seeing that the teachings of Aviation Physiologists are heeded in the theaters of operations. The Personal Equipment Officer is a ground operations position originated by the Medical Service in the Eighth Air Force and trained under medical direction at the AAF School of Applied Tactics, Orlando, Florida. Most of our Aviation Physiologists also have been trained as Personal Equipment Officers, and some are engaged jointly in both fields.

When one understands that from the human standpoint the thousands of missions of heavy bombers over Europe were dependent on oxygen discipline, one may appreciate the value of the Altitude Training Program. The contribution made by the Aviation Physiologist in his altitude chamber and by the Personal Equipment Officer who preaches the gospel of survival in flying units in this country and overseas may be judged from the statistics for anoxia accidents in the Eighth Air Force. The total anoxia accident rate among heavy bomber crew members was cut in the last year from 116 per 100,000 man-missions to 23, a decrease of 80 percent. This record is remarkable. It was achieved in the face of more than a hundredfold increase in man-sorties and an over-all increase in average bombing altitude of 5,000 feet—from 22,000 feet to 27,000 feet. This is *more* significant when one realizes that at 27,000 feet the length of useful consciousness without oxygen added to the air one breathes is less than five minutes; it is considerably longer at 22,000 feet.

The equipment which flyers wear in order to withstand the effects of altitude and the other stresses of flying have come to be called *personal equipment*. The personal equipment problems encountered by an operational air force depend on the nature of its mission, the geography of its operational zone, the climatic conditions in that zone, and the competency, preparation, and experience of the service facilities available to it. The problems encountered, therefore, will vary from one air force to another.

For example, the difficulties experienced by our air forces based in England were conditioned by the great strength and long preparation of the enemy, the hazard of flights over treacherous waters, and the rigorous climate over the northerly latitudes of Europe. Moreover, since these air forces embarked on large-scale offensive operations at a time when some equipment had to be used before it had been completely tested, they frequently served as proving grounds for the final testing of protective flying equipment.

In anticipation of these difficulties a new type of medical organization was set up in the Eighth Air Force—the First Central Medical Establishment. One of its main sections, the Department of Physiology, was charged by General Grow with the testing of all personal and safety equipment used

by flying personnel. The staff consisted of trained air corps and medical corps officers. By field trips and training Personal Equipment Officers, this organization maintained a close liaison with the combat squadrons all over England.

The circumstances calling for establishment of this activity are striking. Briefly, they were as follows:

For a month in early 1943 our air forces saved less than 1 percent of our airmen who were forced to land in the waters about England. At the same time, with greater activity and largely at night, the RAF saved nearly 30 percent of their ditched personnel.

By March of 1943, when our aircraft were forced to go higher and higher to avoid the vigor of enemy resistance, a new type of oxygen system came into widespread use. Because it was novel and required special handling, difficulties arose; one of every nine heavy bombers taking off for enemy territory was forced to return early because of oxygen trouble. With 10,000 man-hours in England alone put into every ton of bombs on the target in 1943, the cost of these aborted flights was staggering.

In the first six months of 1943, there were more casualties to our airmen from frostbite than there were from enemy action. This, too, was a frightening, and preventable, toll.

By development and procurement of equipment, by proper care, and by training, each of these difficulties was overcome. Before I enlarge on the manner by which these and some other problems were solved, I will present the results. By the end of 1943, we were saving nearly 70 percent of our airmen who ditched. On one raid alone, 218 of 221 men forced into the North Sea were saved! Last year, on D-day and D + 1-Day, 82 of 86 troop carrier crewmen were saved, and the four who were lost had been killed or wounded by enemy action before ditching. If circumstances permitted—which they do not—a still more interesting story could be told of our air-sea rescue activities in the Pacific.

Reverting to the early return from oxygen failures, we find that these diminished to such a degree that they were a minor, a very minor, cause of early returns from missions instead of third-ranking cause.

As for frostbites, adequate and proper clothing—especially developed—turned this from the first cause of casualties to nearly the last. While frostbite is of necessity a hazard of every high altitude flight, it diminished from a frequency of 250 per 10,000 men flying to 44 per 10,000. This advance is attributed to a number of factors, including installation of waist-window inclosures in heavy bombers, development of face protection, vastly improved types of electrically heated suits, gloves and shoes, and combined efforts of Flight Surgeons, Personal Equipment Officers, and Aviation Physiologists to indoctrinate airmen in the methods of preventing frostbite.

Before I describe *how* these and some other problems were solved so well, I must refer to another success which probably contributed more to the saving of our flyers' lives in military operational flying than any other single development. I refer to body armor, commonly known as the flak suit. This was employed at the insistence of Brigadier General Malcolm C. Grow, a Flight Surgeon and founder of the Aero Medical Laboratory. As surgeon of the Eighth Air Force in England, General Grow observed that 79 percent of wounds among heavy bomber crews returning from missions over Europe were from low-velocity fragments of common shells and that 85 percent of the fatal wounds occurred in head, neck, and trunk regions which could be protected by armor. He investigated the various types of body armor of medieval England, and with the collaboration of a British sword-maker, developed a quick-release vest, apron, and helmet of shingled steel. The flyers' fear of flak from antiaircraft guns, something against which he cannot fight back, made the body armor immediately popular. Study of a series of cases in which flak-suited flyers were hit by enemy missiles of all types showed that 69 percent were uninjured and an additional 21 percent were only slightly wounded.

Everyone now is quite familiar with the exploits of the B-29 Superfortress in long-range, high-altitude bombing missions against Japan. Aviation medicine played a significant role in the development of this super-

bomber. In fact, Flight Surgeons and Aviation Physiologists, teaming with aeronautical engineers, worked out the basic physiological problems of efficient human performance in the stratosphere long before the Superfortress was conceived.

From the medical viewpoint, the B-29's greatest distinction from previous bombers is the pressurization of its cabins to produce a relatively constant air pressure inside, irrespective of the great reduction of atmospheric pressure in a flight from sea level to the stratosphere. There is, as it happens, nothing new in the idea of compressing the air in a sealed cabin to maintain the partial pressure of oxygen in the lungs. More than 30 years ago, when the heavier-than-air flying machine was just getting off the ground, Cruchet and Moulinier observed with considerable clairvoyance: "As a matter of fact, the [oxygen] problem will never be satisfactorily solved until crew and passengers sitting in an airtight cabin shall breathe at all altitudes an atmosphere practically identical with that at sea level."

Let us review the physiological oxygen principles which give validity to this statement. Air has weight, which at sea level exerts a pressure of 14.7 pounds per square inch of surface. This weight decreases with increasing distance above the earth until at 40,000 feet the air pressure is only 2.7 pounds per square inch. This total atmospheric pressure of 2.7 happens to be slightly less than the partial pressure of oxygen at sea level. As a matter of fact, when the air pressure decreases to below the 10.1 pounds per square inch found at 10,000 feet altitude, the partial pressure of oxygen is no longer sufficient to maintain an adequate arterial saturation of oxygen in a flyer accustomed to living at lower altitudes. Above 10,000 feet the amounts of anoxia to which he would be exposed increase so rapidly that an attempt to breathe free air between 30,000 and 40,000 feet would result in loss of useful consciousness within 15 seconds and in death within a few minutes.

To maintain normal oxygen saturation of the blood at high altitude, the AAF provides the flyer with a standard diluter demand type of oxygen mask and regulator. The oxygen is supplied from low-pressure oxygen cylinders. Actuated by an aneroid and a demand value, the demand oxygen regulator reacts to reductions in atmospheric pressure by automatically increasing the percentage of oxygen in the air breathed by the flyer from the 21 percent found in free air to 100 percent when he reaches an altitude of 30,000 feet. If his oxygen equipment is functioning efficiently, his arterial oxygen saturation is still normal at 34,000 feet and will remain adequate up to 38,000 feet.

Above 38,000 feet even 100 percent oxygen is insufficient for physiological needs, and the accumulative effects of anoxia will develop with prolonged exposure. The only way to raise the flyer's service ceiling beyond this point is to increase the air pressure at higher altitudes.

This may be done with a pressure demand oxygen mask and regulator, which can increase the pressure of oxygen in the lungs throughout the respiratory cycle by 15 to 25 millimeters of mercury. This will prevent anoxia up to an altitude of 42,000 feet and may be used in emergency between 45,000 and 50,000. By maintaining a positive pressure which prevents any possible air leakage the pressure demand mask at altitudes in excess of 35,000 feet will provide, in addition, a margin of safety not obtained in the diluter demand mask. Nevertheless, it has many of the disadvantages inherent in any closed system of breathing. These include the necessity of maintaining a facial fit of the mask which is virtually sealed, the discomfort attending prolonged periods of wear, the possibility of the mask freezing at the subzero temperatures found at high altitudes, the possibility of small amounts of anoxia developing from malfunction, the general fatigue which frequently results from continued use of an oxygen mask, and the restriction of movement by an oxygen hose of fixed length. Moreover, building up pressure in the lungs exerts pressure on the veins carrying blood to the heart. Pressurization of the lungs alone is therefore not without potential danger.

The ideal solution to all this is the development of pressure cabin airplanes, such as the B-29. In this airplane, the forward compartment and the rear compartments are sealed and pressurized. They are connected

by a pressurized tunnel passageway over the bomb bay. In addition, the tail gunner's compartment, a lonely cupola under the rudder, is pressurized. Up to 30,000 feet it is possible to maintain a pressure which provides adequate oxygenation of the blood without the use of an oxygen mask. Thus the greatest practical advantage of pressurization today is to free the flyer from dependence on the oxygen mask at altitudes below 30,000 feet. If the airplane were to continue to climb above this altitude a point would be reached where the interior pressure decreased to the equivalent of 10,000 feet altitude and the aircrew would then be required to put on oxygen masks. In other words, the effect of pressurization would be to jack up the "floor" of the demand oxygen system from 10,000 to 30,000 feet altitude and therefore raise the theoretical service ceiling for this type of equipment from about 40,000 feet to whatever limit engineering places on the airplane's altitude performance.

The practical and potential values of cabin pressurization are numerous. In addition to freeing the flyer from the necessity of wearing an oxygen mask at moderately high altitudes, pressure disturbances of the middle ear, the sinuses, and the gastro-intestinal tract are greatly minimized. Moreover, pressurization prevents aeroembolism, or "the bends," which becomes a problem at altitudes above 30,000 feet. The sealed pressure cabin acts, to a great extent, as a sound proofer, and hence noise levels within the cabin are lowered. Finally, the heat resulting from air compression reduces the hazard of high altitude frostbite.

Low temperature is second only to lowered atmospheric pressure as a physiological hazard in high-altitude flight. Anoxia is far swifter than subzero cold but no less deadly. High-altitude frostbite, principally of the hands, has constituted a leading cause of battle casualties in heavy bomber operations at altitudes of 25,000 and 30,000 feet where temperatures of 40 to 50 degrees below zero are normal. But having indicated already the successes that have attended our efforts in this direction, I shall not dwell further on this subject except for one further point.

In addition to the electrically heated clothes, shoes, and gloves, to which reference was made at the outset, clothing design for AAF flyers has been a problem of major concern. The magnitude of the problem is suggested by the fact that flying clothing in 1942 was designed for aircraft most of which seldom flew to an altitude of 20,000 feet. The heaviest clothing was very popular; it was leather, lined with shearling. As more and more flyers were trained and as the cold temperatures to which they were exposed became lower when flying went higher, and as the duration of exposure increased, the inadequacies of such clothing soon became apparent. For example, men perspired in shearling clothes at lower altitudes and this imposed the danger of frostbite at higher altitudes. As this clothing was worn, the shearling became packed, and this resulted in a marked decrease in insulating efficiency. In short, demands for increased production, without clothing specifications that fulfilled the desired purpose, created difficult problems. Particularly were shoe and glove designs found to be inadequate.

The problem was approached effectively by the Clothing Branch of the Personal Equipment Laboratory at Wright Field. Through cooperation of a group of engineers, anthropologists, physiologists and clothing and textile experts, clothing of proper and effective design was achieved. On one hand, bulk was reduced and, on the other, improved and variable multilayer insulation was obtained. For example, this was accomplished effectively with respect to gloves. This was based on RAF experience. A rayon inner liner serves to protect the hand if outer gloves are removed for brief but necessary use of the fingers; next, a heavier five-fingered glove gives considerable protection, permits moderate movement and use of the fingers, and over these, a heavy lined gauntlet with the index finger free is provided for the long periods of time when such insulation is necessary and the hands need not be actively used.

The use of manufactured pile on the body clothing itself has marked a great advance over shearling. It is less bulky than shearling and, because it is stronger, it resists crushing in a surprisingly effective manner.

By using outer jackets that can be donned over others, the degree of insulation can be controlled by the demands of a situation in a way hardly possible with a single, heavy leather, shearling garment.

These are not idle considerations to the man who must fly. Consider, for example, the fact that, regardless of the ground temperatures, whether in the tropics, in the desert, or in the far northern latitudes, the air temperature over 20,000 feet is about the same at all latitudes, if certain allowances are made for seasonal differences.

The type of aircraft a flyer will use is of great importance in determining the type of clothing that can be worn.. It is well known that fighter aircraft have very small cockpits. The problem is not simplified for a larger than average pilot who must wear a parachute, a life vest, a one-man life raft, a personal emergency kit, and clothing for insulation. It is true that fighter aircraft now have very effective heaters; but in combat these must be turned off as a safeguard against noxious gases. On long-range fighter escort missions at high altitude this has proved to be a real problem. Since operations in the Pacific could be carried out at lower average altitude than the vigor of German resistance permitted, the going with respect to clothes in that theater was easier for both flyers and ground echelons.

I should like to cite two practical instances of the way scientific thought permeates into air forces activities before I leave this subject. On the basis of available data, a chart was prepared by two scientists at Wright Field giving for every latitude and season the type of combination of air forces clothing that will be most effective in combating cold at any altitude up to the ceiling of our present aircraft. This chart, based on the known ambient air temperatures, the insulative characteristics of various combinations of clothing determined by tests made in cold chambers, and upon certain physiological considerations, is incorporated into the Stock List Catalogue of Army Air Forces Clothing.

One of the oldest problems and one of the newest advances in aviation medicine concerns the protection of the flyer against centrifugal force. This problem concerns the fighter pilot rather than the bomber crew inasmuch as it is our fighter and fighter-bomber aircraft which commonly perform steep dives or sharp turns as a part of their tactical maneuvers. The human body is constructed to function at the weight produced by gravity on a body at rest. When the body is projected through space it undergoes acceleration, which means a change in velocity either in magnitude or direction. Decreases in velocity present the pilot with no special problems as long as his airplane continues in a single direction, even if the velocity increases to 500 or even 700 miles an hour. Should something prevent him from continuing at this speed, for example, a mountain side, the problem of deceleration is obvious. There is a less obvious but real difficulty, however, when the airplane is traveling at high speed and merely changes its direction. This may occur in a loop or turn, when the pilot's body tends to continue in a straight line. This centrifugal acceleration is similar to a gravity at right angles to the tangent to the path the airplane is traveling; it will be toward the right if the airplane is turning toward the left and downward if the airplane is straightening out and turning upward from a dive. This centrifugal force, as it is commonly called, may be greater than the force of gravity. Ordinarily a centrifugal force four or five times gravity is about all a man can take without "blacking out." Blacking out is a visual phenomenon due to an interruption of the blood supply to the brain. The pull of centrifugal force operates from head to feet, and when the blood becomes four or five times heavier and tends to pool in the abdomen and legs, it becomes increasingly difficult for the heart to deliver blood to the brain. The heart, after all, puts out no more blood than it receives. Vision is affected first, being so sensitive to loss of oxygen supply that it will black out within four to eight seconds after the blood has ceased to circulate in the retina. At this point the pilot is still conscious, but if the excessive force continues a few more seconds, he will lose consciousness. When the centrifugal force ceases, it takes a pilot only three to five seconds to regain his vision, but if he has lost consciousness, it takes as

much as 15 to 60 seconds to recover. In dive-bombing or dog-fighting either a momentary loss of sight or loss of control can be disastrous. Early in the game, "hot" pilots learned to crouch, tense their abdominal muscles and yell or growl when they pulled back on the stick for a quick change in direction. These reactions helped to increase g-tolerance by making the body more rigid and thus restricting the pooling of blood in the abdominal and leg regions. These maneuvers not only had practical drawbacks but were extremely fatiguing and, through experience, fighter pilots learned not to exceed their individual blackout threshhold. Due to the great advances in aeronautical engineering, this put them far behind their airplanes in performance under the stress of centrifugal force.

Air-minded physiologists have experimented for many years in an effort to develop anti-g protective devices. Prior to World War II the Royal Canadian and Australian Air Forces and the United States Navy developed workable g-suits which in one way or another aimed at the restriction of the movement of blood from head to feet by the application of pressure on the legs and abdomen. In these suits, pressure during exposure to excessive g-forces was produced with water, carbon-dioxide gas, or air. The Navy suit was essentially a pair of tight, high-waisted pants and girdle containing 17 air bladders. It was supplied with a gradient of three pressures. In 1943 this suit was adopted by the Aero Medical Laboratory at Wright Field as the most practicable for tactical use, and experimentation with it was begun. This suit, which weighed 10 pounds, was found to be wholly effective in flight tests but too hot and cumbersome.

The eventual result of experimentation with the centrifuge for humans and in battle tests was a two-pound, single pressure g-suit which became standard wear for Army Air Forces fighter pilots in the European Theater of Operations. It was recently described in the *Readers' Digest* as the Air Forces *zoot suit*. This g-suit, which resembles a cutaway version of a pair of pants, contains only five air bladders placed over the abdomen, the thighs, and the calves. Operated by compressed air from the airplane's vacuum instrument pump through a gravity valve, these bladders automatically inflate or deflate when centrifugal force rises above or falls below 2 g. While the extra g-tolerance provided the pilot averages only about 1.5 g, fighter pilots wearing the g-suit have never reported a complete blackout. The g-suit has made an important contribution to fighter tactics as reflected in the statement of one fighter pilot: "I was never able to turn inside a Jerry before, but I did it today." The tactical advantage applies not only to plane-to-plane dog-fighting but to fast maneuvers in tight formation and to sustained deflection firing while circling. Evasive action during low-level strafing and dive-bombing, particularly to avoid flak, can be carried out more rapidly with the g-suit. In all these maneuvers the pilot can sit up and look around to the side and rear without fear of blacking out. The ability to keep his eye on the enemy is the fighter pilot's best life insurance, and many pilots have provided enthusiastic case histories of kills resulting from the protection provided by g-suits. Some have actually stated they owed their lives to it. The g-suit, which had its first general combat application in the Army Air Forces, constitutes a direct contribution of the physiological branch of experimental medicine to the superiority of our fighting men.

THE sudden and intense forces of acceleration pose a number of problems for those charged with the care of the flyer. These are, generally speaking, the problems attributable to the impact of forced landings on land or sea, those due to bailing out of fast moving aircraft, and those due to landing by parachute.

How is the flyer protected? This depends upon the type of aircraft to which he is assigned. If seated, like a fighter pilot, or the pilot and co-pilot of a multiplace aircraft, the flyer uses a shoulder harness, tightened just prior to impact. This braces the man securely, making him for the moment of impact a part of the seat and even of the cockpit. This not only increases the duration of time over which the impact force acts but it increases very much the area of the body over which these forces act.

It can be demonstrated mathematically that, given a set of hypothetical circumstances, a flyer using a shoulder harness will experience a force of about five pounds per square inch, whereas in the same accident, using the customary lap safety belt, a flyer thrown so that his head hits a gun sight or other projection of about one square inch will receive a blow approaching 2000 pounds per square inch. Despite this advantage in favor of the shoulder harness, there are still flyers who would rather risk the latter consequences than use a shoulder harness with a quick-release device on it.

For the aircrew men for whom no seats and shoulder harnesses can be provided, other protective measures are necessary. The solution is to find a position for every man in which he can brace himself securely. For example, it is desirable to have the man seated with back and head held firmly against a bulkhead. The number of such positions available is limited, and second best positions must be taken. These are established for all air crewmen in every type of aircraft.

The dangers incurred in bailing out of aircraft moving at high speed are considerable. If, for example, a flyer who escaped from an airplane moving at 300 miles per hour pulled the ripcord immediately upon clearing the aircraft, the force acting in the direction of the aircraft would be far greater than when deceleration in this direction is over. In the former situation the parachute may tear; if it does survive the opening shock, a force of considerable magnitude is transmitted to the body by the supporting harness. If this has been improperly fitted, bodily injuries are sure to result. The means of effectively combating these dangers lie in two directions; one by training and the other by assuring that every parachute harness is firmly fitted at the proper points on the body. The latter is a responsibility of the Personal Equipment Officer. Without going into detail, it may be said that these and certain other measures have resulted in a very marked decrease in injuries sustained by our flyers who bail out of aircraft. At one time, statistics show, about 40 percent of our men sustained some sort of injury; now the percentage of accidents—and the general severity of them—is but a small fraction of this number.

I SHALL mention in passing an urgently pressing problem, and one which as yet has defied satisfactory solution despite extended study by the experts. I refer to the subject of seat comfort in long range fighter escort missions. The air crewman in a multiplace aircraft can stand up and move about during flights of many hours duration. The fighter pilot, however, is obliged to sit in the confines of a single seat, always clothed and equipped for bail-out, perhaps over enemy territory, over the jungle, or over water. He is obliged to remain in this fixed position for three to four hours at a time, on some missions. As yet, there is no way to provide even reasonable comfort, and the effect on bodily efficiency has yet to be measured under these circumstances.

IN another direction, the AAF medical services have established methods for testing and improving the flyer's night vision. Whereas the *cones* in the central portion of the retina are the principal organs for distinguishing color and detail in daylight, night vision is largely a function of the *rods* in the outer area of the retina. These rods are 1,000 times more sensitive in dim light than the cones. For most efficient function at night, however, the flyer must adapt his eyes to darkness by protecting them from light for 30 minutes prior to use—a protection which must be continued during the period of flight operations. This is accomplished without the necessity of remaining in darkness by the use of dark adapter goggles containing red lenses. Furthermore, the flyer must learn the off-center method of gazing at objects at night to bring his rod vision into full use and to avoid the night blind spot presented by the center of the retina when he gazes directly at the object. This is done by looking 15 degrees to one side of the object he wishes to see. The flying candidate's night vision is tested as a part of the physical examination for flying. Night vision indoctrination, including simulated firing at targets under moonlight and starlight conditions, is especially valuable in aerial gunnery training.

ANOTHER problem of interest is airsickness. A history of swing, train, sea, or other types of motion sickness is sought in the physical examination for flying, and the aviation cadet is continually observed during training for susceptibility to airsickness. Actual flight constitutes the best index. Study conducted at the School of Aviation Medicine based on the use of a swing test and upon reports of the relationship between airsickness and individual fear of flying have disclosed that the syndrome is a product of two factors: motion and emotion. Airsickness primarily due to motion has important differences from the preponderantly emotional type. In the former, which occurs only during rough weather or acrobatics, nausea is relieved by vomiting, symptoms disappear upon landing, and an immunity is acquired. The emotional type may occur before or after take-off and produce nausea not relieved by vomiting, sickness during smooth flight, headache, and symptoms after landing. Sixty-five percent of navigation cadets and 30 percent of pilot cadets experience airsickness early in training, but only about 10 percent of all navigation cadets and 1 to 3 percent of pilot cadets are eliminated because of airsickness. The symptoms of those eliminated fall predominantly in the emotional category and are linked with fear of heights, or of flying and other factors impinging upon emotional stability. Conditions commonly associated with airsickness, such as current diet, aircraft odors, temperature, vibration, and the sight of others being sick, have been discounted as having any stimulus in airsickness other than lowering the threshold at which disturbances of the sense of equilibrium may produce symptoms.

Space is lacking to go into several interesting subjects, e.g., the efficiency with which research engineers of some well-known manufacturing concerns have constructed oxygen regulators that take into account individual variations in rate and depth of respiration; how they have allowed for the limited tolerance of the human body to overcome resistance during inspiration in order to operate a demand valve for hour after hour, whether resting or periodically active; how they have provided oxygen of increasing richness, in accordance with the necessity to maintain a given partial pressure of oxygen in the lung alveoli. Our engineers, working in accordance with the specifications laid down by physiologists, have produced a regulator that meets the rigorous requirements for maintenance of bodily efficiency and that satisfies the aircraft designers by providing a system which, for the amount and pressure of oxygen carried, is of minimum weight and adaptable to such distribution of weight as the engineers require.

It would be interesting to discuss the progress which is being made in the field of cockpit design and standardization, and the mutually advantageous cooperation between our armed services by the setting up of Army-Navy Standards. It is true that some of these developments are aimed at simplification of manufacture and procurement, but whenever these engineering developments are considered, a staff of medical officers, aviation physiologists, and other specialists are consulted in order that human capabilities, reactions, and limitations will be evaluated. And so it is with considerable satisfaction that many biological scientists, both medical and nonmedical, can survey the success of the application of their work in the notable combat record of the Army Air Forces in the face of novel circumstances.

FUNCTIONS AND OPERATIONS OF THE NATIONAL ROSTER[1]

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A GENERAL paper about the National Roster by President Leonard Carmichael of Tufts College (for more than four years Director of the Roster) was published in this Journal in February, 1944. The purpose of this article is to give a more intimate account of the actual operations of the Roster and the changes which have taken place in its functions during the five years of its existence.

The first purpose of the Roster was that of an inventory or registry that, as completely as possible, would record and classify the scientific, technical, and professional personnel of the nation. This purpose has been accomplished to as great a degree as could have been hoped for, and the National Roster became and remains a repository of more complete and up-to-date information on such personnel than is available from any other single source. The file of the Roster contained on December 31, 1944, approximately 440,000 registrations. The distribution by broad general fields is given in percentage in Table 1.

A second function of the National Roster from its inception has been placement. It is estimated that during its five years of activity the National Roster has made referrals of 170,000 registrants for commissions in the Army and Navy and for civilian positions in government and war industries. No detailed attempt was made to follow up these referrals, but it is estimated that about 50,000 of these positions were filled by the efforts of the Roster. This type of work still continues. The reconversion period will probably see a considerable increase in the placement activities of the Roster. All members of the Armed Forces who are qualified in the fields covered by the National Roster and who are seeking assistance in locating suitable employment are to be referred to the Roster as they pass through the separation centers and return to civilian life. They will be registered with the Roster if not already enrolled, and their records will be made available to employers if they are seeking employment.

A third function of the Roster may be summed up as fact-finding. As indicated in Dr. Carmichael's article, the Roster has been called upon to prepare reports on the personnel situations in physics, engineering, chemistry, geology, mathematics, and the agricultural and biological sciences. It has also evaluated the needs of industry for engineers and scientists and has surveyed educational institutions to acquire data necessary for the training programs of the Armed Forces. Findings of the Roster have been used by the Essential Activities Committee in connection with Selective Service deferments. In addition to several bulletins giving results of surveys, there have been written in the Roster a number of descriptions of

TABLE 1

THE PERCENTAGE DISTRIBUTION OF ROSTER REGISTRANTS BY BROAD GENERAL FIELDS

Engineering		46.2
Mechanical	13.2	
Civil	10.7	
Electrical	9.5	
Chemical	5.9	
All others	6.9	
Physical Sciences		24.5
Chemistry	16.2	
Mathematics	3.3	
Physics	2.6	
Geology and Geophysics	1.9	
All others	0.5	
Management and Administration		13.2
Social Sciences		6.9
Agricultural and Biological Sciences		5.9
Architecture and Planning		2.1
Languages		1.2
		100.0

[1] Published by permission of the Director of the National Roster of Scientific and Specialized Personnel, War Manpower Commission.

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professional fields, and handbooks outlining the detailed duties in each profession.

The Roster has had a varied but important part in advising the Selective Service System regarding the deferment of professionally trained men necessary to essential and critical activities. Approximately 17,000 such cases were referred to the Roster for recommendations either to State Directors or Local Boards of the Selective Service System. A like number of student deferment affidavits were processed, examined, and approved by the Roster during the short-lived deferment program of 1944.

The mechanics of National Roster operations indicate a systematic and thorough utilization of professional personnel records. It is of interest to trace the progress of an individual record as it is recorded and used. The original questionnaire is exceptionally complete and thorough to meet every possible need and to answer every possible question of those seeking specialized personnel. In addition to approximately two dozen specific pieces of information, the registrant gives on a detailed check list not more than four special fields in which he has competence. Each profession is divided into a number of main groups and these in turn subdivided into specialties. For example, a registrant indicates that his principal professional field is Organic Chemistry (Code Number 408). Under that heading the major division 4, Theoretical Organic Research, is selected and under that heading Organometallic Compounds (40.8.84) is checked.

When an individual questionnaire with its check list is received, all data are coded by well-trained operatives and are then transferred to a special 80-column punch card (Holerith card). From these cards two index cards are made and from one to four qualifications cards. One index card is put in a statistical file, and this file is used only for compiling general statistics as to registration, distribution by fields, extent of education, age, etc. The other index card goes into an operating field and is used to locate and assemble individual records according to any coded item. The qualifications cards are identical with the other index cards except for the order of the coded fields of proficiency. For example, if an individual is primarily an organic chemist in the field of synthetic drugs but has had some experience in polarigraphic methods of analysis, two qualification cards would be punched so that this man's record could be located under either heading. A log card shows the registrant's name, address, and serial number and these cards are filed alphabetically. Whenever an inquiry is made about the registrant, his name and address are sent to the "log room" and the number of his record is obtained. This is sent to the questionnaire file room, and the folder, containing the individual's questionnaire and all subsequent correspondence and records, is received.

When a call is made by a government agency or other organization for an individual of certain qualifications (age, education, experience, foreign language, etc.), the index cards are mechanically searched and a complete list of registrants meeting these qualifications is printed. The folders for all these individuals are sent to the placement staff for briefing and for use in communicating with registrant and prospective employing agency for arranging interviews and the usual placement procedures. In the case of requests from industry or other private employer, the only records selected are those of registrants who have indicated that they are available for employment.

Statistical information from registration records is gathered mechanically from index cards without dealing with any registrant as an individual. Only in cases of detailed study are the questionnaires examined. For example, the question may have been raised as to whether foresters are employed in the aircraft industry because opportunities in their own field were limited or because their special knowledge of wood was being utilized.

The Roster plans to carry on the functions outlined in this article, with continual adaptations to the peculiar needs of the period of reconversion and the succeeding peace and to render the greatest possible service to government, education, and industry and to the professions on which they depend.

THE LOGIC OF THE LOST YEAR

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In the August issue of the Scientific Monthly there appears an article by Professor A. G. Keller of Yale University presenting a strong case for a national, compulsory, peacetime service bill. It seems only fair that some rebuttal be offered. In summary Professor Keller's arguments are as follows: The training will be military, but not narrow or harsh. It will be physically beneficial for the trainees. It will toughen but not brutalize their mental and moral fiber. It is essentially education—education in discipline and democracy. The so-called lost time for formal education can be avoided by deleting unnecessary training in grade school, high school, or college, or by accelerating. The training is necessary for defense. Without it, "we deserve what is coming to us."

Military training need not be harsh but it is necessarily narrow. It is a training in the tactics of offense and defense in organized warfare and is as specialized in its approach to a trainee as a master mechanic in his approach to an apprentice. Moreover, as has been abundantly pointed in numerous articles and speeches, the training received during the next few years will be outmoded when and if the next war comes. If we may assume that it will take at least as long after this catastrophic disturbance to get ready for a new war as it took after World War I, the proper time to start training young men for defense is in 1962. Had we begun compulsory military training in 1937, 23 years after the start of World War I, we would have been prepared for World War II when it came, except that we should have had to revise all the tactics after we recognized the new weapons and methods employed by the Germans.

If, on the other hand, the training is not to be for organized warfare, it is difficult to see how it contributes very directly to defense. If it is to be "vocational," it is a far more expensive method than that of providing federal scholarships to vocational or professional schools. Our experience with the misfitting extensively practiced by the army at present lends little encouragement to the notion that men will be treated individually or assigned to tailor-made or even altered-to-fit jobs.

Military training, largely physical, would undoubtedly improve the health and physique of most trainees. It is to be expected, however, that the details of a service act would provide for a physical examination before induction and a subsequent weeding out of the "physically unfit." Only the good will be accepted and made better, whereas those who really need corrective measures will have to rely upon whatever advice and assistance is afforded by government agencies for civilians or upon such aid as the man or his parents may have the desire and the money to employ. As for the "limitation of intoxicants," army life at present seems to indicate that those who wish to drink will find the means to do so, temperately or otherwise. Has the army reformed the less desirable citizens who have been inducted in this war? Would the army accept trainees from a reform school then any more willingly than it accepts convicts now? Aside from obvious and admitted crime such as the murders and the large-scale racketeering which have occasionally been outrageous enough to crash the censored headlines, is it not true that army life teaches a man to provide for himself by becoming an accomplished scrounger? Is there any reason to believe that a peacetime service corps, without the patriotic stimulus of "saving the homeland," will be any less addicted to skimping, shirking, cheating, passing the buck, and, very occasionally, in addition to these normal rear-area practices, indulging in a spot of drunkenness, murder, or rape?

And, of course, discipline. Appalling as is the lack of discipline in modern education there seems to be a swing away from the extreme laxity of a few years back. It

would be futile to deny that 20 years in the army would impress upon a man a strong habit of obedience to those authorized to order him about. It would be equally futile to maintain that a single year of army life would establish a firm habit of submission to authority, one which would survive the man's discharge from the corps and make him a less rebellious workman, student, or citizen. The imposition of unaccustomed discipline and restrictions almost invariably produces a rebellious reaction and a determination to make the most of freedom when it comes, in this case at the end of twelve months. How do furloughed soldiers behave now? Those who used to celebrate by painting the town red still do so, insistently. Those whose habits were originally less exuberant and more law-abiding celebrate inoffensively. However, we must agree that a little training in discipline is better than none and that some training in it, either in civilian education or in a service corps, is clearly indicated.

Like Scrooge's sister, education in democracy has always been a delicate flower that a breath might have withered, but, also like Scrooge's sister, it usually survives its frailties long enough to produce a hearty and likeable offspring,—but not if regimented. Indoctrination (nee Propagandizing) in Democracy could and, of course, would be carried out efficiently and well in the training camps—far more intensively and successfully than it could in the civilian educational system. We should remember, however, that democratic procedures are just what the army does not employ. The authorities which must be obeyed (and with no right to strike) are not elected and are under no supervision by those who must take the orders. Army life stresses uniformity, conformity, and unquestioning obedience to authority imposed from above. Democracy is moribund unless differences, heretics and cranks, and constant criticism of delegated authority abound.

There are many brands of Democracy. More than superficial differences distinguish the Democracy of which Soviet Russia boasts from that in France, England, or Sweden, and even these last are not identical with our own. Our Democracy calls for a minimum of federal supervision. The current army manual is always *right*, and the Democracy taught by army methods would be unlikely to permit, much less to encourage, questioning and differences of opinion. Teaching millions of young men a selected system of democratic principles is getting close to that regimentation we wish to avoid. All-in-all, it is very questionable whether the trainees would emerge from their army boarding school with a much more trustworthy political philosophy than they had when they entered it, and it is certain that civilian education, with its discussion groups and wide variety of opinion, has a far better chance than the army has of imparting to the citizenry a genuinely democratic viewpoint.

The mingling of young people of different social and racial backgrounds *should* certainly promote understanding and cooperation, which are the essence of successful practical democracy. But we must not forget that the terms "kike," "wop," "nigger," "hunky," and the like are used just as freely in the public as in the private institutions and that race riots take place as frequently in the North and West where segregation is not semilegal as in the South where it is. There are plenty of cliques and snobbish clubs in public as well as in private schools. In any large mixed group each person mingles by preference with those of most congenial background. Companionship in a fox-hole or a small army post is one thing; mingling in a large training camp is quite another. The possible benefit to Democracy derived from the heterogeneity of the group is probably vastly overrated.

To justify his thesis that the "lost year" could be made up without genuine loss Professor Keller presents a rather scathing criticism of the state of American education. It is pseudocultural. Some courses are boring. Much time is "wasted." If these items of little or no value were omitted, a year might easily be saved. (Having saved it, of course, the proper procedure is to spend it at once upon a year of compulsory training.) As Professor Keller says, no educator

is ever satisfied with the way in which education in general and his own part in it in particular are being handled. Any educator would admit that some useless material is taught and that some material is presented without being taught at all. It is the effort to decide just *what* is badly taught or superfluous that causes bitter feuds within the academic walls. No schoolboy, no college student, knows *certainly* what he will be called upon to do in life. His well-laid plans may be the very proposals of which God will dispose by filing them in the cosmic waste basket. Neither the pupil nor the teacher can tell definitely just what is superfluous, although any pupil will state categorically what he *considers* useless and any teacher can list certain courses (taught by others) which are mere hangovers, with academic inertia as their only excuse for existence. Certainly for those students who enter college with only a vague notion of what they intend to do with their lives, or whose plans involve college solely as a cultural experience, it would be entirely impractical to select the "must" subjects.

Army training would not be faced with this difficulty. The army knows precisely what it wants the man to be trained for: a specific job in a combat team, a supply depot, a communications group, and so forth. The army spokesmen say categorically and with feeling that they do not want that twelvemonth of specific training to be split up into four three-month summer camps. They want one continuous period of twelve months because that is the efficient way to train men. This is precisely the argument which the educators of preprofessional and engineering students have been urging. To interrupt the man's technical training, which is arranged in a carefully integrated and sequential whole, is to make that training far less effective. Also, engineers, doctors, and scientists are admittedly far more useful to a general defense program when they work in their own fields and utilize their specialized education than when they are shifted to a new technique for which their training has given them no background. It is surely more wasteful to snatch them from their valuable education for a year of vegetation than it is to pad college curricula with 15 or even 30 semester-hours of superfluous or worthless material.

It could be argued that *all* formal education at the college level, which would be the level most affected by the proposed program, aside from preprofessional training, is wasteful or at least a mere superfluous luxury in a social sense. There are certainly many men in college who, through lack of natural ability or lack of motivation, have already reached the limit of their formal education and are so much deadwood on the campus. And there are many who use their college courses to prepare them for creative work in literature or art, activities the intrinsic value of which is sometimes denied by the "practical" man. But these students too have a strong urge to obtain a diploma, even if some of them decline to do more than the absolute minimum of work required to obtain it, and to remove them from formal study for a year is to risk ending their college careers. Study habits are very easily lost and are regained only with difficulty. Men who have been forced to interrupt their education and subsequently try to resume it find the going very hard. Many of them fall by the wayside. Where is the farsighted wisdom in making these men and others forfeit their diplomas and what those diplomas stand for on the ground that they will be serving their country by undergoing a training in techniques which will be outmoded before they are used? It would be a tragic and enormous waste of a somewhat rare commodity, intellectual ability.

If, in spite of the weighty arguments against it at this juncture, a compulsory training bill is inevitable, at least let us avoid the ruinous loss of trained and trainable minds which we have suffered in this war by our short-sighted policy of avoiding the appearance of class distinction and inducting our preprofessional students as though the greatest service they could perform for the war effort was to fire a gun or drive a truck. Let there be exceptions made for college men with a certain standard of scholastic attainment. And that does not mean proficiency on the gridiron. Nostalgic references to the coach as the man who

taught me the most that was really worth knowing are good inspirations for sentimental doggerel, but a man's ability to earn the leisure in which to reminisce depends upon his performance in the classroom and study-hall, rather than in the game in which he helped to trounce the traditional rival.

Whatever may be the moral effect upon the trainees who pass through the program, the mere institution of the plan will have a profound effect upon the morality of the general public. Under the stress of perfervid wartime patriotism, we have submitted, for some five years now, to a long list of restrictions. We have been drafted, we have been rationed, we have been taxed. Even in this period of submission we have had much draft-dodging (some half-million cases are said to have been investigated to date), plenty of black market, and ample tax-evasion. When it comes to donating a year of one's own or one's child's life to one's country, not in an emergency but just as a general precaution, who can estimate the extent of the lying, evasion, string-pulling, and skulduggery that will be evoked? Even now we know of draft boards rather softhearted about granting deferments, a bit lenient in interpreting directives, or a little forgetful, always of particular names on the list. What will it be like then? Or are Americans so superior to the citizens of those nations which have struggled with compulsory service for so many years that every young American of 17 will draw himself to his full height, look the Procurement Bureau in its collective eye, and say in a voice hoarse with emotion: "I regret that I have but one year to give to my country?" Of course most American boys would comply voluntarily, if grumblingly, but it would be just those actual and potential juvenile delinquents, the dead-end kids and the like, the ones that are to be benefited by the program, who would find ways to dodge.

The logic of the lost year is that, having allowed our emotions to assume command and having instituted immediately a training program vastly expensive in years as well as dollars, after some five to ten years of peace and moderate prosperity, no one will want the program any more except the Army and those Cassandras who foresee the next war. By popular demand it will be discontinued at the very epoch when it might begin to become useful. Wars do not come overnight. It is only the final act of violence that arrives with startling suddenness. If our Cassandras could become sibyls and command the faith of the public, our training program could begin years hence but in ample time. The tragedy of it is that if the program is not put through in the heat of war it will probably take the commencement of violence in the next war to produce it.

The fact remains that to pass a national, compulsory, peacetime service bill at this juncture would mean a lost year for every one of the millions of trainees, and, especially for those of college caliber, it would threaten the stultification of several subsequent years. Not one lost year, but many!

A PHILOSOPHER'S REPLY TO A SCIENTIST'S ETHIC

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THE leading article of the April 1945 number of THE SCIENTIFIC MONTHLY (Vol. LX, No. 4, pp. 245–253) is entitled "Ethicogenesis" by Chauncey D. Leake, vice president of the University of Texas Medical Branch in Galveston. This well-documented and thought-provoking paper is "a deliberate attempt to establish an ethic on the basis of scientific knowledge and in scientific terms." (253)* To substantiate his thesis for a scientific ethic, Dr. Leake uses two interrelated arguments which we may call and state summarily as follows:

The Methodological Argument. The "descriptive" approach of modern science should replace the "normative" approach of traditional philosophy in the field of ethics if the possibility of ethical science is to be realized. (247)

The Darwinian Argument. Goodness should be measured in terms of its "survival" value to the human species. (248)

Leake starts off rather cavalierly by charging that, "to the professional scientific worker, discussions on morals by professional philosophers usually appear to be metaphysical irrelevance in the face of our present knowledge" (245), and by making an earnest plea that philosophers henceforth follow the example of Spencer, James, and Dewey, and apply the scientific method to all fields of human endeavor.

In view of the general fact that most philosophers have been seriously challenged by Leake as a professional scientist, and in view of the particular fact that the article, "Philosophy in a World at War," published in the March 1942 number of *Fortune* by my former teacher and present colleague William P. Montague, Johnsonian Professor of Philosophy at Barnard and Columbia, is specifically accused by implication of "metaphysical irrelevance" (245), I therefore think it is our duty to answer the plaintiff without apology and fear of becoming a *persona non grata* to the republic of science. As devil's advocate, I shall speak for Montague and try to show that each of the foregoing arguments for establishing an ethic on the mere basis of "biological evidence" (253) is subject to serious and relevant criticism. Since my subsequent remarks constitute a "rejoinder," I shall concentrate on the polemical angle of the questions at issue and only touch lightly on those points about which I am in substantial agreement with the author.

CRITIQUE OF THE METHODOLOGICAL ARGUMENT

To begin with, we wish to side wholeheartedly with Leake's plea that we should apply the scientific method to "all fields of human activity and interest" (245) as much as possible. No thinking individual today can deny that our robust faith in science means that we ought to make the most out of it and not just be half-scientists. However, our support of this needed extension of the scientific method in practice does not commit us at all to his peculiar emphasis as to what the method of science is in theory nor to his over-confident trust in its all-sufficiency. *Caveat lector!*

According to the author, the general procedure of science may be defined essentially as the "descriptive approach." It is strongly urged throughout the article under consideration that such approach should be applied to ethics and be substituted for the "normative" or metaphysical approach of the "classical ethics of philosophical thought." (247) This conclusion may be taken as the initial phase of his methodological argument for a scientific ethic.

To the above phase of the methodological argument there are two logically possible

* The number in parenthesis following a quotation from "Ethicogenesis" is the number of the page of the SM from which the quotation was taken.

answers, one from science and the other from ethics. Let us now consider them in turn.

The scientific answer to the author's explicit claim, which appears at the close of the methodological preface to his moral analysis, to wit, that the "scientific method of arriving at the 'truth' is now well defined in theory and practice" (248), is our counterclaim that, in spite of the fact that he knows what the scientific method is in *practice,* he does not have a clear and distinct idea as to what it is in *theory.* He senses, to be sure, that science is not merely an empirical *description,* however accurate, of the so-called "raw" facts, but a rational *explanation,* however tentative, of them as well. He knows almost by instinct that the scientific method in general is *rational observation;* that is, neither observation nor reason *alone.* (248) Yet, he keeps putting the accent on the "descriptive" approach throughout his whole discussion and talks as if the observational or the experimental element were the only "characteristic of scientific effort" (247) or the only factor of importance in the process of science. Of course, such Baconian emphasis is psychologically intelligible as a critical weapon to combat blind authoritarianism and idle speculation, but it is not logically justifiable, since a good definition of the scientific method must include *all* its essential attributes.

Moreover, Leake is aware of the importance of *"experimental reasoning"* in the "way of mathematics," but at the same time he isolates the quantitative type of analysis from the "way followed in the natural sciences" (248), and thereby fails to appreciate that "modern science really dates from the union of the experimental and mathematical methods of procedure,"[1] as Montague insists. In other words, the author keeps the two ways of knowing so separated that he does not seem to realize the important role of the mathematical chains of reasoning *within* the very realm of the natural sciences. A mere glance at the mathematical structure of modern physics should convince one of the intimate *union* of the two ways. Whether we accept or not Galileo's metaphysical faith that "the Book of Nature is written in mathematical language," modern science at least accepts the workable methodological faith that reason is written in mathematical language. Although it may be difficult for a pharmacologist like Leake to swallow such mathematical pill, the relentless logic of modern science, which he enthusiastically professes, may well nigh compel him to take his own medicine for consistency.

To summarize, our first criticism of the author's methodological analysis is that he pays too much attention to just one aspect of the general pattern of science and thus tends to commit the fallacy of exclusivism. We say "tends" deliberately in fairness to the fact, which we have already admitted, that he senses the necessity of a broader definition of the scientific method.

As the first objection points to a deficiency of commission, the second points to one of omission. The author omits a discussion of the connecting link of the two alternating stages of the empirical-rational and the inductive-deductive cycles of scientific inquiry, namely, "constructive imagination," whose important part in the formation of the hypotheses of science was appreciated in the last century by John Tyndall in his famous lecture on "The Scientific Use of the Imagination." Despite its evident importance, Leake dismisses intuition as "little more than logical inductions made so rapidly that the maker is not conscious of the steps in the process, until they are analyzed." (252) The fact that this doctrinaire dismissal occurs in a passage concerning religious "revelation" may throw some light on why intuition is scientifically suspect to him and why constructive imagination is the missing link in his conception of science.

The omission of the imaginative factor in scientific theory is rather surprising for a professional scientific worker like Leake who admires Charles Darwin so much. Didn't Darwin arrive at the epoch-making theory of evolution through his constructive imagination "brooding over the facts" of biology and working on his memories? The history of scientific discoveries, both great and small, shows in the concrete and in varying degree that constructive imagination (not the irresponsible and idle kind, of course) is, so to speak, the bridge between the empirical and

rational poles of scientific procedure. To be sure, all imagined hypotheses in science must be tested by experimentation, whenever and wherever possible, if they are to lay any claim to intellectual validity, but they must *first* be *proposed* before they can later be confirmed or refuted by experiment. This initial step is precisely the work of constructive imagination. As Montague is fond of putting it: "In short, we may say that imagination proposes and reason disposes."[2]

What we have previously called the initial phase of Leake's methodological argument for a scientific ethic can be divided into two parts. The first part *affirms* the validity of the "descriptive" approach in general; the second part *denies* the validity of the "normative" approach to ethics in particular. The foregoing brief survey of the fundamental components of the general method of science constitutes our answer to what he affirms. We shall now present in what follows our answer to what he denies. If we succeed in indicating the exact nature of the "normative" in the field of ethics, we shall thereby rescue this most basal of the categories of moral understanding from his irrelevant attack. Our subsequent task is to demonstrate that it is impossible to talk ethics without the category of the "normative."

In any search for knowledge there are two distinct problems to be granted priority: one refers to the purpose of the investigation and the other to the class of objects to be investigated. In other words, any intellectual injury involves a *for what* and a *what*, respectively.

From the standpoint of aim, inquiries may be divided into two general groups, *existential* and *normative*. An existential inquiry, by definition, is interested primarily in studying objects as they *actually are;* a normative inquiry is concerned mainly with studying objects as they *ought to be* or with evaluating objects. Ethics, by definition, is necessarily a normative pursuit because it aims at determining systematically whether a special class of non-sensory objects of human conduct or character has or has not the specific type of value designated as moral. Whereas, for example, biology is a study of life as it exists in nature, ethics is an examination of the *good* life, whether it exists or not.

From the standpoint of content, objects as such have a determinate structure to which the mind in its cognitive effort directs its attention. Now just as there are two groups of inquiries with respect to aim, so we find on examination that there are correspondingly two general classes of objects, *fact-objects* and *value-objects*. The objects of existential inquiry are simply called facts, those of normative inquiry are called values. The essential difference between biology and ethics, with respect to subject matter or object matter, to be more exact, is that the former studies life as a series of biological facts and the latter studies the good life as a series of moral values. Although it is possible to investigate moral matters as facts, they then fall within the scope of the social sciences and not strictly within the scope of ethics proper. In brief, it is only when moral facts are analyzed and appraised as values that ethics is appropriate to them. This conclusion establishes the *autonomy* of ethics as one type of normative inquiry.

Before going any further, it is important to note that the previous distinction between the existential or "is" kind of inquiry and the normative or "ought-to-be" kind is based on the classification of *aim* and has nothing to do with the question of approach or the *how* of inquiry. There is no such thing as the "normative" approach implied by Leake's whole methodological argument against "the classical ethics of philosophical thought." (247) The normative is relevant to the *for what* and the *what* of inquiry but irrelevant to its *how*. Hence, the author's conclusion that the "descriptive approach" should replace the "normative" one is based on the false premise that there is a normative method to replace in the field of ethics. As a matter of fact, the descriptive approach is common to both existential and normative inquiries. The difference between the two does not lie in that one describes and the other does not, but in the *what* each specifically describes. The fact that the descriptive approach has yielded better results in existential inquiries than in normative ones has

no bearing on the present point at issue. Moreover, when he observes that classical ethics is "dogmatically 'normative' and has scarcely considered the 'descriptive' approach characteristic of scientific effort" (247), he is guilty of committing not only the fallacy of false premise, but that of misplaced status in shifting the locus of the "normative" from the teleological and ontological contexts, where it properly belongs, to the methodological context, where it does not. This confusion of contexts, which is found in writers who classify inquiries on the basis of aim into "descriptive" and "normative" instead of into existential and normative, is the cause of the dislocation of the proper perspective on the moral situation and is responsible for Leake's not seeing that the normative is *intrinsic* to ethics. For ethics without the "normative" end in view and "norms" as content is a contradiction in terms. Philosophical ethics, if you like, may be bad on other grounds, but to indict it for being "normative" is similar to indicting science for being "scientific."

If our reasoning has been cogent, the conclusion follows that ethics *ut sic* is a normative affair with respect to both aim and content. This conclusion denies the validity of C. H. Waddington's position, which is accepted by Leake, namely, "that ethical judgments may be statements of the same kind as scientific statements" (249), since ethical judgments are normative statements concerning values and *not* of the same kind as scientific statements concerning facts. A principle of morals differs from a principle of nature as gravitation, for example, in two major ways: (1) a moral principle such as justice can be violated without destroying its validity, but a physical principle cannot; (2) a principle of nature tells us what is so, but a principle of morals tells us what is desirable and not necessarily so.

Lest our defence of the normative character of ethics be misunderstood as "metaphysical irrelevance," let us hasten to note that the radical difference between moral and scientific statements does not imply that ethics should formulate its principles of conduct independently of the available knowledge of human nature supplied by psychology and other pertinent sciences. It is obvious that a sound ethic for mankind must come to terms with the basic facts of human nature, but it should be equally obvious that all such facts put together do not add up to the system of morality we should live by. We should begin, of course, with the facts of human nature, but not end there. This empirical starting point is to act as a springboard to the vital business of ethics. The *ideal* ought to be certainly connected with the *real* in order to be relevant to the human scene, but ought also to be carefully distinguished from the real in order to be valid. As Montague expresses the relation of the two realms of discourse: "Existing situations do, of course, determine which values are relevant to their time and place, but success or failure in realizing the appropriate ideal neither creates nor destroys the validity of that ideal."[3]

The foregoing passage also serves to discredit the author's misinterpretation of Montague's profound belief in the Platonic vision of absolute values. Leake may be right in affirming that science "finds no evidence for the objective existence" (245) of the absolute principles of Truth, Beauty, and Goodness, but he is wrong in implying that Montague believes in their "objective existence." Now the latter's argument for their absolute value is not based on their alleged objective existence or *reality* in the ordinary sense, but rather on their genuine objective validity or *ideality*. After all, Montague tempers his Platonism with common sense, and his faith in absolute ideals not only prevents him from making believe that "what is is right," but also prevents him from making believe that "what is right is."

To return to our analysis of the good from the normative standpoint, the "ought" and "ought not" (or their moral equivalents) are not in truth the wishful "rationalizations" sanctioned by traditional philosophers, as the author suspects (247), but rather the axiomatic polarity of any ethical inquiry. Morris R. Cohen is right in holding that any moral system must by definition start with an "indemonstrable assumption" in terms of which everything else within the system is demonstrable, "since we cannot

have an *ought* in our conclusion unless there is an *ought* in one of our initial assumptions or premises."[4] The essential difference between Kant's "categorical imperative" and what we may term Leake's "biological imperative" lies precisely in the *specificity* of the "ought," not in that the first has an "ought" and the second does not have it, because the Leakian conclusion that we "ought" to survive is just as much an "ought" as the Kantian conclusion that we "ought" to universalize our conduct. To be sure, the sceptics and their not so extreme brethren the relativists, like the author, in their "natural reaction to the absurd claims of moral absolutism," are justified in "insisting that there is an arbitrary (in the sense of volitional) and indemonstrable assumption in every moral system," but from this it by no means follows that "moral systems contain nothing but assumptions or that all assumptions are equally true or equally false."[5] We still have the important job of determining *which* of the possible imperatives is the best guide for mankind.

Moreover, we may or may not agree with Kant's monumental "categorical imperative" as a moral principle, but we certainly cannot dismiss it as "religiously a matter of persuasion, of exhortation." (247) For there is a big difference between the "exhortations" of a Moses and "the great intellectual effort of Immanuel Kant." (247) As we might put it in language familiar today, Moses worked for Jewish *morale* and Kant wrote for a universal *morality*. And speaking in behalf of a Moses, don't we need morale and exhortation at times when things are going tough? Be that as it may, the real problem of reflective morality is not to do away with the "normative," which is a logically impossible undertaking, but to determine as adequately as possible what we "ought" and "ought" not to do *concretely* in any given ethical situation. And as there is no royal road to truth, there is less of a one to goodness.

So much, then, for what we have called the initial phase of the author's methodological argument in behalf of a "descriptive approach" for a scientific ethic and our two answers to it, (1) the scientist's answer derived from the general nature of the method of science and (2) the moralist's answer derived from the general nature of the field of ethics. Let us now proceed to examine its ultimate phase.

The ultimate phase of Leake's methodology is expressed in the belief which he shares with Julian Huxley and A. J. Carlson that "the answer to the question of the insufficiency of science is more science." (245) From this basal premise it is inferred that the scientific approach needs no supplementation of "metaphysical or supernatural considerations" (245), that philosophy is alas! dead, and the "devotions" of metaphysicians, in the funeral speech of Hugh Miller, "are a wake, administered to a corpse." (246)

Apart from the rejoinder that theologians may offer to the above indictment of superfluity, we as philosophers must ask: How can the body of philosophy be resurrected from the apparent death imposed by the successful invasion of the natural sciences which have been developed so impressively in the modern world?

To meet this death charge, let us consider, first of all, the logical problem of the definition of terms. If "science" is extended to mean "knowledge," then, of course, the original premise turns out to be a truism, to wit, the answer to the question of the insufficiency of knowledge is more knowledge. There's no argument about that! Nevertheless, this still leaves open the problem as to whether "science" in the modern sense of the term and "knowledge" are completely identical in connotation and denotation. Although "science" literally (from the Latin) means "knowledge," it is misleading to identify the two terms outside of the etymological context, especially since the history of modern science has prided itself so much in its persistent efforts to distinguish its type of verifiable knowledge from that of so-called common sense and philosophy. All this may seem rather trite hairsplitting on our part, but it is important for what is to follow to have disengaged Leake's contention from its truistic form, about which there obviously can be no argument.

In order to prove that science in the modern sense is not enough in our quest for truth, let us consider what would be the ideal of knowledge. Perfect knowledge, like an organism, would have a double aspect. On the one hand, it would be as deep and as broad as possible, and on the other hand, it would be as detailed and as particular as possible. The former properties constitute the *whole-aspect* of the body of knowledge, the latter its *part-aspect*. In other words, our intellectual goal is to know both *everything* as a whole and *every thing* down to its minutest detail. To be sure, that is what we would like to have, but we hardly need a reminder of how far we are from such goal. We fully realize that the deeper and broader our knowledge of things is, the less are the chances of its being certain. As the extensive element of richness of content increases, the intensive element of certainty decreases. But we should likewise admit the reverse of this principle. The more detailed and particular our knowledge is, the less are the chances of its being significant. As the intensive element of certainty increases, the extensive element of richness of content decreases. The intellectual life, like any life, must sacrifice something and pay its price for what it holds dear. In brief, selection always implies exclusion, whether in extension or intension.

Returning to our biological metaphor, knowledge-of-the-whole,—that is, the deeper and broader kind of knowledge, is not the simple sum of knowledge-of-the-parts, the detailed and particular kind, just as a living organism is not merely the simple sum of its parts. Integration in the calculus of knowledge is not ordinary addition. Moreover, the tragic element in the human drama of knowledge lies in the fact that growth in one aspect of knowledge is not usually accompanied by a corresponding growth in the other. The intellectual story of mankind is a revelation in the concrete of a "cultural lag," to speak in the sociological language of W. F. Ogburn, between the two aspects of the cognitive situation.

We usually give the name of *wisdom* to the deeper and broader type of knowledge and the name of *information* to the detailed and particular type. Now, how can we characterize the man of wisdom? Vergilius Ferm aptly states: "A person is said to have wisdom when he is able successfully to peer beneath or beyond the superficial and seemingly self-evident, to see subtle relationships, and when he knows how to co-ordinate seemingly isolated items and view them in their true perspective and to bring this insight to bear upon the problems and tasks which confront him. A man may have much knowledge and not be wise, however, he could hardly be wise without adequate knowledge. Wisdom is more than information; it is information crowned with understanding."[6]

From the standpoint of our intellectual heritage, science is the discipline whose aim is to give us information and philosophy is the discipline whose aim is to give us wisdom. Science is the search for organized and tested information about the *parts* of the universe in which we live. Philosophy is the search for the best wisdom about the whole universe and man's place in it. Wisdom, by definition, is not *less* than but *more* than information, and "touches *all* the scientific interests and *all* experience."

Philosophy literally (from the Greek) means: the love of wisdom. Several years ago I published a paper[7] where I attempted to develop the far-reaching implications of the etymological definition of philosophy, which is immortalized in Diotima's Tale of Plato's *Symposium,* and suggested therefrom the difference between philosophic knowledge or wisdom and scientific knowledge or information. This difference in *type* of knowledge implies a methodological dualism or a difference in *method.* To acquire scientific knowledge we require scientific method; to acquire philosophic knowledge we require philosophic method. Since wisdom is a knowledge with two dimensions, breadth and depth, we need insight or *vision* for its broader dimension and reflection or *understanding* for its deeper dimension. A two-dimensional type of knowledge necessitates a two-fold approach for its attainment, one for each of the dimensions. Vision and understanding are to philosophic method what observation and proof are to scientific method. As science proceeds, in John Dewey's termin-

ology, by "analytic observation," so philosophy proceeds by reflective vision.

Leake practically agrees with the logical positivists that philosophical propositions or propositions about wisdom are nonsense (he is a bit kinder in accusing them of "metaphysical irrelevance"), since they can neither be confirmed nor refuted by the strict and restricted canons of science. However, this negativistic claim in itself does not prove that they cannot be tested *by other means* nor does failure to prove mean necessarily disproof. If we make a blanket assumption from the start that philosophical theories are not verifiable at all, we are committing ourselves unduly to an intellectual defeatism which is not warranted by the actual and possible facts of the case. For just the verification of a *single* philosophical theory would be needed to destroy the validity of that blanket assumption. And if this possibility were ever to occur in actuality, it would be an unfair change of names to contend that, *before* the test of verifiability that theory was philosophical and that *after* the test it became scientific. After all, we should not put all our eggs in one basket and leave the poor philosopher "holding the bag."

Moreover, the evident fact that philosophical propositions are, on account of their more comprehensive and fundamental nature, much more difficult to verify than scientific propositions should not serve as an obstacle to putting them to as many tests as we can possibly muster. Since the organism of knowledge implies a *uni*verse or one world, there is no *a priori* reason to believe that the methods we find so successful in explaining the parts of this world, cannot be applied with some degree of success at least to the explanation of the whole. In a monistic world, the parts must reflect the whole to which they belong, like Tennyson's "flower in the crannied wall," and the whole must in turn be reflected in its parts. Hence this parts-whole connection in *reality* makes possible a legitimate transfer of *method.*

The possibility of the transfer of the canons of science to the field of philosophy suggests that the various scientific tests at our disposal can and should be employed to eliminate the latter's unfit hypotheses and thus indirectly pave the way for establishing the survival of the fittest ones. This eliminative function of the different scientific tests in the realm of philosophy, though negative in character, is highly important because, by rejecting the false alternatives to any philosophical issue, we have solved at least one half of the problem of attaining wisdom. The final and positive test for determining the validity of philosophical propositions or theories, after the rigid preliminary tests of science have performed their function of elimination as effectively as possible, is or should be the universal experience of the ages and the sages which is embodied in man's deepest understanding and broadest vision. Such ultimate test is the *something more* which science as such can in no wise furnish.

Philosophy and science, in being concerned with two different aspects of the cognitive situation, are mutually supplementary rather than logically incompatible, just as an organism as a whole and its particular organs are not necessarily in conflict. The fact that philosophy and science differ in content and method does not make them mutually incompatible, just as the fact that man and woman differ in sex does not make them incompatible with each other. This is the logic of the situation. Now the historical fact that some philosophers and scientists have been at war, as is so poignantly manifest in the article under consideration, means simply that they have not appreciated the powers and limitations of their distinct business. Philosophers and scientists should not be at war, especially since their common spirit, the critical attitude, and their common logic to which they make constant appeal, are by their very nature *self-correcting.* All of which history reveals the comical side of the human drama of knowledge. The fault, of course, lies not with philosophy and science as such, but with some mentally myopic philosophers and scientists.

Philosophy needs as little justification as science. They are both human business in this precarious life of ours which demands some kind of intellectual adjustment to a baffling environment of problems, old and

new. "Metaphysics," in Montague's beautiful words, "whether a good or a bad thing, is at least a necessary thing in the sense that the urge to it is irresistible. Accepting humbly and gratefully from the sciences their verified discoveries of the truth of this and this and that and that, we must still ask, *What is it all about?* What is the nature of things, and where do we come in? To most of the people some of the time and to some of the people most of the time, such queries make an appeal that will not down. And not until the noble capacity for *wonder* has deserted the human soul will metaphysics cease. And after all, what is there to be afraid of? A metaphysician has nothing to lose but his claims, and a world to gain—a world in which there is the certainty of that solemn joy which comes only from meeting and grappling face to face and hand to hand with the greatest problems of existence; a world, furthermore, in which there is always the possibility of extracting from the products of creative imagination some nuggets of deep and precious truth."[8]

In accepting Hugh Miller's funeral pronouncement on the status of metaphysics, Leake is consciously or unconsciously committing the generalization fallacy. To argue that *all* metaphysics is dead because *some* metaphysical systems are dead is like arguing that *all* physics is dead because *some* physical systems are dead. A thing perennial as metaphysics may be pronounced dead prematurely, but it certainly does not stay dead. Whether we like it or not, metaphysics will out, even in the minds of those who without serious reflection pronounce it a "corpse." Philosophical systems may come and go, but philosophizing as an intellectual activity will continue so long as man retains his sense of wonder about the whole scheme of things. And as long as man is faced with those fundamental problems which affect his weal and woe, his thirst for ethical wisdom will never be quenched.

In the light of the above considerations, we wish to bring to a close our critique of the ultimate phase of the author's methodological argument for a scientific ethic by affirming that "the answer to the question of the insufficiency of science" is not only, as he over-confidently believes, "more science," but also *more wisdom*, the aim of all philosophy worthy of that name. For scientific information, no matter how complete and certain, is no real substitute for philosophic wisdom, no matter how incomplete and uncertain. Wisdom is irreplaceable. As is unfortunately confirmed by our daily experience, more science does not necessarily bring more wisdom. We certainly have more experimentation and gadgets at our control than the ancient Greeks ever dreamed of, but are we wiser? Man does not and shall not live by science *alone*. To be sure, we need more science, but we need more wisdom too, in order to live *significantly*.

To avoid possible misunderstanding of our position, we must bear in mind that we have not really attacked the author's faith in science as a noble pursuit of truth. *De scientiā non disputandum.* What we have in essence found fault with is not so much his *science* but his *scientism*, the "ism" which makes science the all-sufficient tool of knowledge and regards all else as "metaphysical irrelevance." In other words, we have criticized what he *denies*, not so much what he *affirms* with respect to the scientific position and its implications. Scientism, a new name for the old positivistic way of thinking popularized in the last century, is both an inadequate philosophy of science and an incomplete philosophy of values. In short, our objections have been more to Leake as scient-*ist* than to Leake as *scientist*.

Critique of the Darwinian Argument

We would like to begin our commentary on Leake's Darwinian argument for a scientific ethic by stating that we fully agree with his general thesis that a system of ethics to be sound and relevant to human conduct must have a *naturalistic* foundation. Nevertheless, this agreement does not commit us to his Darwinian brand of naturalism. As I have defended elsewhere,[9] a thoroughgoing naturalistic philosophy does not lead to the popular conclusion usually drawn from it and implicitly expressed in the article under consideration. The incorporation of man into nature, to be sure, naturalizes man, but this very incorporation also humanizes na-

ture. Leake quotes in agreement with Darwin that the difference in mind between man and the higher animals in the scale of nature is "one of degree and not of kind." (246) His acceptance of the Darwinian statement indicates that he is missing the whole point, which is that this difference of *degree* makes all the difference in the world between man and the rest of nature. The continuity of man with nature, his home, means not only the alliance of ethics with physics and biology, as John Dewey insists (250), but also its alliance with metaphysics and theology. For how can we envisage an ideal of human life without the illumination of a philosophy and the inspiration of a religion? All the talk about the atoms and the monkeys, however enlightening and quickening, is certainly not enough to guide mankind. In short, as F. J. E. Woodbridge observed, a critical and a discriminating naturalism demands not only an "emancipation from a traditional conception of man," but also an "emancipation from a traditional conception of nature."[10]

Moreover, to formulate a "naturally operative ethical principle," which is Leake's aim, "independently of metaphysical implications or considerations" (252), which is his claim, is logically impossible. Any naturalistic interpretation of man's place in the universe, whether valid or not, is as metaphysical in scope as anything can be, since it involves the general question of man's relation to the universe as a whole. The author seems to be so afraid of "metaphysics" that he prefers to call it by another name, "science." He appears to be a victim of the nominalistic fallacy in the literal sense, and what he impatiently kicks out of the front door of his mind surreptitiously slips in from the back door.

It is appropriate to recall at this point that Leake is engaged in a "deliberate attempt to establish an ethic on the basis of scientific knowledge and in scientific terms." (253) In other words, what he proposes to give us is a scientific ethic. Now, since every *giver* of knowledge has a *given,* let us examine briefly what is assumed in his analysis.

Two interconnected assumptions, one major and one minor, are made throughout the article under consideration with respect to the formulation of an ethical principle. In the first place, the author assumes that the "scientific knowledge" relevant to ethics is "biological knowledge" (248) and, in the second place, he assumes that the "biological basis for ethics" is a "corollary of the Darwinian principle of evolution." (252)

Both of these assumptions are subject to the internal criticism of science itself, not to speak until later of the external criticism of philosophy. On the one hand, the major assumption will not be acceptable to that group of non-biologically minded scientists who would prefer to establish a scientific ethic on the basis of physical or chemical knowledge or on their combination rather than on the basis of "biological knowledge," and would claim for their argument that, if one is going to "approach the problem of ethics in a scientific manner" (245), one might as well go from the biological level down to the bottom of things. This first group of critics constitutes the "fundamentalists" of science. And on the other hand, the minor assumption will not be acceptable to that growing group of non-Darwinian biologists who would prefer to establish a scientific ethic on the basis of a non-Darwinian principle of evolution. This second group of critics constitutes the "protestants" of contemporary biological theory, since the mechanists still predominate in biology. The present disagreement among the biologists themselves on whether Darwinism is a complete explanation of evolution, J. B. Pratt warned,[11] should caution us against accepting it as gospel.

What is most surprising in the article under consideration is that Leake as a present-day physiologist should accept the nineteenth-century Darwinian interpretation of evolution without taking into account the various modifications it has been obliged to undergo under the impact of recent findings in the field of biology. Such omission tends to over-simplify evolution as a law of biology, with the fatal result that a ready-made and sterile formula to fit all the cases is substituted for a flexible and fruitful search to discover its empirical evidence. And the fact that "evolution" has been in vogue is

no prior guarantee of its scientific infallibility.

Of course, any appraisal that may be submitted of the *given* in Leake's ethical system does not impugn at all his right of making assumptions on which his conclusions depend. No thinker can work without presuppositions. However, this logical right of the free thinker involves the corresponding duty of making the most valid assumptions. In order to prove that Leake's Darwinian assumption is not the most valid one for ethics, let us now state his argument and consider its moral consequences.

The author's Darwinian argument for a biological ethic is that, since any living thing to survive must adapt itself harmoniously to its environment, the "good" for human life is precisely whatever promotes such "harmonious adaptation." (250) In the words of S. J. Holmes: "Morality becomes just one phase of the adjustment of the organism to its conditions of existence." (250) On this view called the "harmony theory" (248) of ethics, the moral enterprise is simply one of discovering those human relationships which are "mutually satisfying" (252) to people and consequently have "survival" value. "Adaptation toward the goal of mutual satisfaction" is recommended as the basis of an effective system of "biological engineering." (250)

The "naturally operative ethical principle" is stated in terms of a concomitant variation of "survival" and "mutual satisfaction," as follows: "*The probability of survival of a relationship between individual humans or groups of humans increases with the extent to which that relationship is mutually satisfying.*" (251–2) This "proposed statement," which is claimed to be "derived from objective and agreed upon biological evidence" (253), is admittedly a "corollary" or "special case" of the more general Darwinian principle of evolution: "The probability of survival of individual, or groups of, living things increases with the degree with which they harmoniously adjust themselves to each other and their environment." (252)

Before examining the moral implications of the Leakian outlook, it may be questioned whether its ethical principle is a legitimate "corollary" of the Darwinian theory of evolution. If evolution is conceived strictly in Darwinian terms as a "struggle for existence," then it follows that the ethical principle most compatible with it is not the "Golden Rule" of Jesus and Buddha, which seems "to have been devised in appreciation of the naturally operative ethical principle" (252–3) defended by Leake, but what we might christen the "Iron Rule" of Nietzsche and his followers. Leake is too much of a Spencerian to draw the right conclusion from his Darwinian premises. His compromising type of morals is too tame to be consonant with the Darwinian picture of Nature "red with tooth and claw." Hence we contend that the "Iron Rule" of the "Superman" theory rather than the "Golden Rule" of the "harmony theory," is more in keeping with what the Darwinian Huxley[12] of the last century figuratively called "the gladiatorial theory of existence."

Now if we do not accept the ruthlessness of the "Iron Rule" on moral grounds, then we should not logically accept the Darwinian principle of evolution in its original form on metaphysical grounds. We cannot have our cake and eat it. Herein lies precisely the dilemma of Darwinian ethics: Nietzsche's "Iron Rule" is *immoral* and Leake's "Golden Rule" is *incompatible* with its metaphysical foundation. If "the ape and tiger methods of the struggle for existence are not reconcilable with sound ethical principles," as Huxley[13] vigorously charged, then we must do what he failed to accomplish, namely, find a better explanation of evolution which would fit *all* the facts of life and would make intelligible the difference between the life of man and the rest of living things. And in order to achieve a sound conception of nature and man, it is not enough to argue with Spencer that, if the ethical process is "part and parcel" of the cosmic process, then the two cannot be put in opposition, because we must still explain their evident opposition within the single continuum of evolution.

However, it is at least to Huxley's credit that he saw the tremendous incompatibility between the ground and consequence of the

ethics of evolution from a Darwinian standpoint, even though his Promethean defiance of Nature was evidence of his moral sensibility rather than proof of his ability to solve the problem on the intellectual plane. In any case, no matter what form the ethics of evolution may take, the statement of its principle can hardly be "emotionally neutral" (253), as Leake claims for his own case. Neutrality is no more possible or even desirable in ethics than it is in politics, where vital issues are often at stake. In such delicate matters we have to choose sides, and it is to be hoped that all available knowledge be our guide in choosing the better side, if we would live like rational animals.

There are three principal and closely connected moral consequences to the author's Darwinian thesis concerning a "biological basis for ethics." (251) They are (1) ethical relativism, (2) ethical egoism, and (3) ethical practicalism. What we propose to do in what follows is to indicate the weaknesses of each of the foregoing doctrines. Let us examine them in the order mentioned with primarily this end in view.

Leake prefaces his conclusion that our moral categories are "relative" by agreeing with C. H. Waddington's discussion of four anti-intellectualistic tendencies which have contributed to the prevailing and highly fashionable belief in relativism. These "four lines of thought" (249) are psychoanalysis, comparative anthropology, Marxism, and logical positivism. In spite of his uncritical acceptance of these four "attacks on the intellectual validity of ethics," Waddington has faith in the possibility of a science of ethics based on "the course of evolution," since he "feels that ethics is based on facts of the kind with which science deals." (249) Leake "feels" likewise but supplies no proof. And what is worse, both men fail to realize that the logical outcome of the fourfold attack robs not only "ethical statements of any claims to intellectual validity" (249), but eventually robs all scientific statements of such claims as well. For the logic of relativism inherent in the four attacks, which have penetrated deeply into our contemporary climate of opinion, is more fatal to the intellectual validity of science than to that of ethics. To argue with the relativist that ethical beliefs "have no general validity" (249) is bad enough as a guide to morals, but to argue that scientific beliefs have no general validity is even worse as a guide to science. Of course, Leake is too much of a good scientist to be an extreme epistemological relativist, even though he ambiguously identifies "the scientific and relativist approach" (245) at the beginning of his paper and later says that "truth" is "relative" in the sense that it is continually "subject to revision," adding that "goodness" is also "relative" in the same sense. (251) In spite of the fact that his terminology at times is misleading, he does not defend the relativist conception of truth when he affirms: "What we mean by the truth is the opinion *agreed to by all who investigate*, and it is the object represented in this opinion that is the real." (247) However, if we are not mistaken, he does not seem to realize the chaotic and defeatist implications for both science and morals of the four anti-intellectualistic or relativistic "lines of thought" with which he sympathizes.

The irony of it all is that he "feels" protected from the cancerous growth of relativism in our midst today by holding fast to the Darwinian theory of evolution, when it is precisely the extension of that theory to knowledge and conduct which has been partly responsible at least for such growth. Historically speaking, the tragedy of Darwinism is that it has contained the seeds of its own destruction because it has not only given birth to a legitimate child, the science of biology, but also to an illegitimate one, the "Frankenstein" of relativism. In other words, Darwinism as biological relativism is sound in so far as we possess a sufficient amount of empirical evidence on the evolutionary character of the realm of nature, but it is unsound when it is extended to the realm of logical and moral meaning and is transformed respectively into epistemological and ethical relativism. In their half-ardent and half-cynical misapplication of evolution from the realm of existence, where the evolutionary process properly belongs, to the realm of essence, where it does not, the relativists hopelessly confuse not only the variant

things of nature with the invariant relations of logic, but also the changing content of moral rules with the relatively permanent form of moral principles.

Since we have just indicated the weakness of relativism, let us pause for a moment to consider its strength.

The general value of relativism, whether applied to knowledge or conduct, is a *negative* one. Epistemological relativism is a good antidote to dogmatism and authoritarianism in matters of opinion. As we had occasion to point out in the first part of our paper, the strength of moral relativism lies in its acute awareness of the variable character of human creeds and codes, and thus constitutes a sound reaction to the inconsistent claims of moral absolutism. This experiential fact of the variations and inconsistencies of our moral judgments is what makes the debate of "absolute or relative criteria" for "goodness" and "right" so acute and not "irrelevant," as the author summarily dismisses the issue (251), and hence calls for a reasonable way out. What reconciliation of this old but crucial debate is possible, is out of place in our rejoinder.

We turn now to an appraisal of the second of the moral consequences to Leake's Darwinian argument for a "biological basis for ethics," which we previously called ethical egoism.

Leake states: "From a consideration of our biological knowledge, the implication is clear at once that survival for an individual living thing or for a particular living species, is 'good' for that individual or that species." (248) This statement is an expression of biological egoism, the doctrine that self-preservation is the first law of nature.

Such conception of self-preservation looks self-evident but is really misleading when applied to human beings. M. R. Cohen remarks: "Men generally have a positive preference or urge to live and want to postpone the pain of death. But they also want certain things for which they willingly shorten their lives by hard work, risks, etc."[14] This means, from the positive side, that mere "survival" is not so "good" if it holds no promise in store and, in fact, could get very boring, and from the negative side, it means that "non-survival" is not so "bad" if it makes for achievement and significance. From the foregoing it follows that survival and non-survival, strictly speaking, are *morally neutral,* meaning that biological categories as such are irrelevant to ethical appraisal. Thomas H. Huxley appreciated the difference between the biological and moral set of categories when he pointed to the fallacy of ambiguity in the Darwinian phrase "survival of the fittest," about which "hangs a moral flavour."[15]

Biological egoism leads naturally to ethical egoism, the view that self-interest is the first law of morals. This view on examination suffers from a tautology. If we admit that "survival" or self-preservation is "good" for the human organism, the question is still left open as to what *kind* of "self" is to be preserved. Montague clarifies this issue in the following way: "Let us suppose that we have each of us decided that the highest moral good means *my* highest self-interest; what kind of self shall I become? Shall I make my ego the sort of ego which finds its attainment of power or life-fulfillment in narrow or physical happiness, or in broad spiritual happiness? In domination over others, or in cooperation with others? Admitting that the pig at the trough and the martyr at the stake are each actuated by self-interest, which of these self-interests shall I prefer and strive to attain? It is no answer to say, 'Whichever is to my greatest interest,' for that is like saying, 'It is to my interest to seek what is to my interest.' What is required is a criterion or principle for deciding what kind of self-interest is the best."[16]

The above logical difficulty would still hold if we were to shift from "personal" to "social satisfaction." (250) The change would be quantitative and would not thereby affect the qualitative problem underlying all hedonism, whether egoistic or altruistic, whether of a single individual or of a whole group. In the latter case we would have to decide what kind of group-interest or what kind of "social satisfaction" is the best, independently of its "survival" value. Furthermore, the fact that a human relationship, like the

husband-wife partnership, is "mutually satisfying" (252) to the parties involved, is no final guarantee of its moral worth, no matter how high its "survival" value. Once more we would be obliged to determine what kind of "mutual satisfaction" is the best.

And finally, let us consider the third of the moral consequences to the author's Darwinian argument in behalf of a "biological basis for ethics," namely, ethical practicalism.

Any appraisal of the "adaptive factors" (248) of moral life in particular necessarily involves an evaluation of the Darwinian conception of life in general.

We believe that Montague presents a valid criticism of this view in his essay entitled "A Materialistic Theory of Emergent Evolution," in which he holds: "It seems to me that it is a great mistake to measure vital excellence by degree of adjustment to environment. If life be at all as we have described it, its business is not to adjust itself to the environment, but to adjust the environment to it, to impose its pattern upon its surroundings and increasingly inform them. It is the inorganic systems that tend toward conformity and orthodoxy and approximate in their increase of entropy more and more to undifferentiation and equilibrium, thermodynamic or otherwise, with their environment. The deader a thing is, the more stable its adaptation to its *milieu.* A block of granite, a diatom, a clam, an ape, a Socrates, embody in increasing measure an aggressive and rebellious power to impose their retrospective and impliedly prospective patterns upon a neutral or more or less hostile world. This invasive, insurgent, and heterodox temper of life does not, of course, preclude- on the contrary, it necessitates- a certain modicum of adaptation. Life must stoop to conquer; but unending conquest, not conformity, is its goal."[17]

If Montague's position is true of "vital excellence," it is even more true of moral excellence. Moral life as *moral,* by definition and observation, cannot be measured by its degree of adjustment to environment, but rather is or should be measured the other way around. And the fact that moral life as *life* requires a certain amount of adaptation to environment is no reason for making a virtue, as Leake insists on doing, out of the necessity of our organic predicament. To be sure, we need *natural* life to achieve the *good life,* but to this truism we are tempted to reply with a "so what"?

Ethical practicalism, the doctrine defended by Leake and other Darwinists that "our moral systems represent ways by which we adapt ourselves to our environment" (249), is in reality the ethical correlate of biological practicalism, the doctrine that moral customs originated as useful instruments of adaptation to environment and therefore, as S. J. Holmes holds, "one potent reason for their adoption is their conduciveness of survival." (250) However, in spite of the important contribution of biological practicalism to "ethicogenesis," ethical practicalism suffers from the genetic fallacy in evaluating moral systems in terms of their historical origins as ways of coping with the environment. The common origin of moral codes does not determine their validity. And the fact that human beings began to be good in order to live is no reason why they should not later be willing to die in order to be good.

Leake's so-called "harmony theory" of ethics is anxious to preserve the "balance of nature" by means of a biocentric type of equilibrium, in which the function of morality is to help the whole human species adjust itself harmoniously to its environmental conditions. In view of the various difficulties of his position, we should like to suggest the opposite kind of "harmony theory" in ethics, based on an *anthropocentric* type of equilibrium, in which the function of morality is to help make the environment of fact conform to the will of men so as to achieve the greatest possible good for all. If the "good" life is to mean more than the "practical" life, it is necessary to reverse our usual tactics of coping with the environment and reach a stage of interaction opposite to the biocentric type of adaptation.

One final comment in closing our reply. The author recommends that we extend the "burnt hand example" (251) to the whole field of morality. To be sure, the practical demands of daily life oblige us to obey the commands of our environment and learn

from experience the lesson of "The burnt child dreads the fire." But, we may ask: Is the *burnt hand* example enough for the *good* life? Tell it to Socrates! Tell it to Jesus Christ! Tell it to John Huss! Tell it to Giordano Bruno! And last but not least, tell it to the Marines!

In short, our most serious objection to Leake's "harmony theory" of ethics is not so much that it "will tend eventually to lead to a *status quo*" (253), as he anticipates, but that it lacks a Promethean spark and is not of the stuff out of which our moral dreams and heroes are made. Without this Promethean or heroic spark to illuminate and inspire the dream of a better world on earth, man may very well survive in the flesh at the terrific cost of perishing in the spirit.

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[2] *Ibidem*, p. 34.
[3] *Ibidem*, p. 171.
[4] Cohen, M. R. *Reason and Nature*, p. 434.
[5] *Ibidem*, p. 434.
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[7] Romanell, P. What is Philosophy? *Sophia*, 7: 245–251. 1939.
[8] Montague, W. P. *op. cit.*, p. 11.
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[11] Pratt, J. B. *Naturalism*, p. 62.
[12] Huxley, T. H. *Evolution and Ethics*, p. 33.
[13] *Ibidem*, p. 7.
[14] Cohen, M. R. *Op. cit.*, p. 432.
[15] Huxley, T. H. *Op. cit.*, p. 32.
[16] Montague, W. P. *Op. cit.*, p. 570.
[17] *Ibidem*, pp. 433–4.

EVOLUTION

In chaos, primeval chaos,
Came a shifting, a stirring,
The making, the forming,
But in darkness, steaming darkness.

Through the void, formless void,
Quivered a breaking, a rending,
A seeking, a searching,
Then flashed light, living light.

Through the marshes, slimy marshes,
Came a creeping, a growing,
A moving, a spreading,
For there was life, teeming life.

In rolling time, boundless time,
With struggling, with changing,
Out-reaching, up-looking,
Forth strides man, creative man.

—FOREST C. DANA

NATURAL HISTORY IN THE ARMED FORCES

A RÉSUMÉ OF SOME RECENT LITERATURE, MOSTLY BOTANICAL, OF INTEREST TO SERVICEMEN

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THE plants and animals of the regions which our armed forces occupied or expected to occupy in World War II were of much concern to both those planning operations and the personnel who carried them out. Hence, many books and articles concerning them were prepared officially and privately. Some have never been made public, others are available to servicemen only, and still others are available to the public but are scarcely known outside the offices where they were prepared. Because many of these publications could be of greater use to the public and to the servicemen than they are now, the more useful ones are discussed here along with the various trends in interest which brought them about.

The trends in military interest in botany and zoology varied with the changing aspects of the war, but the first concern was for the protection of the men from the dangerous elements in nature, as the poisonous plants, snakes, and other pests of all sorts. Simultaneously, knowledge was sought of the emergency foods that nature could provide for castaways, and the shelter and needed emergency equipment that could be improvised from plant and animal materials. To supply these needs there have been issued several books and literally hundreds of articles and booklets, comprising what is commonly called survival literature. A list of these has been issued under the title *Survival—A Selected Bibliography*,[4] in which are given data on their character and general contents and the issuing and distributing agencies.

Gradually the demand for this type of literature has been supplied. One of the first and most comprehensive in the botanical field was *Emergency Food Plants and Poisonous Plants of the Islands of the Pacific*[21] by E. D. Merrill, director of the Arnold Arboretum of Harvard University. Probably the most widely distributed of these publications was *Survival on Land and Sea*,[12] prepared for the U. S. Navy by the Ethnogeographic Board and the staff of the Smithsonian Institution. Nearly a million copies were printed, but it has only recently been made available to civilian institutions.

Coordinate with this concern about aspects of nature in connection with survival was interest in plants for camouflage to hide military equipment and movements, and in the identification of plant formations, especially from the air, in order to determine the nature of the terrain, a matter of great importance in carrying on military operations. Secret files doubtless contain much information on these subjects, but little has been made public. The Smithsonian Institution published in 1942 *The Natural History of Camouflage*[13] by H. Friedmann, curator of the Division of Birds, U. S. National Museum, in its War Background Series. Here are discussed the ways in which nature uses concealment and the principles which can be applied in warfare.

A little later our military forces became interested in the exploitation of the natural resources of occupied and to-be-occupied areas, and sought information on economic plants which could provide construction and other needed materials. At first almost all such information, when issued, bore the designation "restricted" or even more confining terms, but gradually these limitations to general distribution are being removed and such publications are becoming available. The Bureau of Yards & Docks, Navy Department, issued in 1944 *Native Woods for Construction Purposes in the Western Pacific Region*,[15] by J. H. Kramer, a Yale forester on the staff of the Foreign Economic Administration in Washington. It is a manual containing descriptions and illustrations of important trees and is intended for the use of servicemen engaged in supplying our

forces with construction materials in the Solomon Islands, the Bismarck Archipelago, the Molucca Islands, Celebes, and the Philippine Islands. A second book by the same author, recently issued, is *Native Woods for Construction Purposes in the South China Sea Region,*[16] covering the rest of the East Indies and the countries around the South China Sea.

Much interesting and valuable information is available in *Notes on Forests and Trees of the Central and Southwest Pacific Area*[28] by W. N. Sparhawk, mimeographed by the U. S. Forest Service. A sketch of the forests and principal useful trees of Japan is to be found in *Japan—Forest Resources, Forest Products, Forest Policy,*[29] compiled by the same author and issued in the same way. The Philippines are similarly treated in two as yet restricted publications of the Allied Geographic Section, Southwest Pacific Area: *Special Study of Timber Resources of the Philippine Islands,*[1] and *Vegetation Study of the Philippine Islands.*[2]

Besides this official interest in survival and the natural resources in the war area, there has been much personal interest in the plants and animals on the part of the men and women in the army and navy. Because their deep-seated cravings to know about the things of nature around them could hardly be met officially by the army and navy, scientists in various other organizations have come forward and provided many books, booklets, pamphlets, circulars, and articles on various aspects of the natural history of the Pacific region. The Smithsonian Institution early in the war initiated its well-received series of War Background Studies, a few of which are on natural history subjects. Most of them deal with geography and peoples, with incidental mention of plants and animals. Already noted is one on camouflage in nature. Another is *Poisonous Reptiles of the World: A Wartime Handbook*[9] by Doris M. Cochran, which bridges from the survival literature. The last of the series so far issued is *The Aleutian Islands: Their People and Natural History (with keys for the identification of the birds and plants)*[10] by H. B. Collins, Jr., A. H. Clark, and E. H. Walker. It was designed to interest and inform the servicemen in the Aleutians concerning the wild life of their environment and has already greatly stimulated natural history study and collecting. Recently it has been partly supplemented by the issuance at one of our bases in the Aleutians of a mimeographed booklet *Flowers of Island X*[34] by Lt. G. B. Van Schaack, USNR, an enthusiastic amateur botanist. It contains descriptions of many of the commoner plants based on the author's collections and field notes made to assist in the preparation of the botanical part of the Smithsonian's study on the Aleutians. We can well expect an increasing number of publications by servicemen in various parts of the world, recording their natural history observations. A list of the plants and animals observed by the naturalists in the Army on Kiska Island in the early days of occupation was issued there in mimeographed form by Capt. G. A. Ammann and is now being extensively revised for issuance by the Department of Botany of the University of Michigan. Other notes by servicemen have been published in various periodicals. In the Marianas the military government is reported to be definitely promoting natural history collecting and study as part of the recreational and morale-building program. Such activities are sure to play a part in the programs of our troops in occupied countries.

The U. S. National Museum has issued a series of seven mimeographed circulars on the natural history of the southwest Pacific.[33] The groups treated in these unpretentious circulars are plants, birds, butterflies, mollusks, reptiles, starfishes, and sea urchins and their relatives. Although intended originally to supplement the answers to the many letters received by the Museum asking for information on the flora and fauna of this area, these circulars have filled a real need for general information basic to a pursuit of more detailed information.

Still another valuable series of natural history books and booklets was initiated and sponsored by the American Committee for International Wild Life Protection, commonly called the Pacific World Series. The staff members of the American Museum of Natural History in New York contributed most of the contents of the first book entitled

merely *The Pacific World.*[26] The material was brought together and edited by Fairfield Osborn of the New York Zoological Society. It concerns mostly the geography and peoples with some mention of the plants and animals, containing especially a tabulation of the distribution of the mammals and birds. Others of this series already issued are *Animals of the Pacific World*[8] by T. D. Carter and G. H. H.Tate and *Peoples of the Pacific World*[14] by F. M. Keesing. Others to be issued soon are *Fishes and Shells of the Pacific World*[25] by J. T. Nichols and P. Bartsch, *Insects of the Pacific World*[11] by C. H. Curran, *Plant Life of the Pacific World*[22] by E. D. Merrill, and *Reptiles of the Pacific World*[18] by A. Loveridge. All these have been or are being issued in clothbound editions by commercial publishers for the general public and in cheaper paperbound editions by The Infantry Journal, Washington, for servicemen only.

The birds of the Pacific region have been treated in numerous serial articles, especially in the popular *Audubon Magazine.* Three outstanding books with excellent illustrations have been issued, *Birds of the Southwest Pacific: A Field Guide to the Birds of the Area between Samoa, New Caledonia, and Micronesia*[20] by Ernst Mayr, *Birds of the Central Pacific Ocean; A Popular Account of the Sea-birds and Shore-birds of the Central Area of the Pacific Ocean*[7] by T. M. Blackman, and *Birds of Hawaii*[24] by G. C. Munro. These leave little to be desired in the matter of popular bird books on those areas, so far as available data are concerned.

The fishes have also received treatment. In 1944 in response to the popular need there appeared *Hawaiian Fishes: A Handbook of the Fishes Found Among the Islands of the Central Pacific Ocean*[32] by S. W. Tinker. It has been very popular with the fishermen in our Pacific forces.

These several publications and many others on the natural history of occupied lands were made necessary by the lack of available published works suited to the needs of servicemen. Many scientific treatments of the plants and animals of the Pacific region have been issued in the past century, as well as a very few more popular ones, but most are out of print and not available for distribution. Some reprinting and translating of older works has been done. The Netherlands Information Bureau has duplicated an article revised from the *Handbook of Netherlands East Indies* (1930) entitled *Flora and Fauna of the Netherlands East Indies*[3] and has also duplicated an article by E. C. J. Mohr, *Climate and Soil in the Netherlands Indies,*[23] which is a chapter in his book on soils of equatorial regions. If the need could have been foreseen and the funds had been available at the beginning of the war, many other works could have been profitably reproduced. The Arnold Arboretum has recently published a description of the vegetation of New Guinea entitled *Fragmenta Papuana [Observations of a Naturalist in Netherlands New Guinea]*[17] by H. J. Lam. This is a translation of a series of Dutch articles originally published in 1927–29. It is of special interest to those naturalists of our forces so fortunate as to be stationed in this botanically rich and little-known country.

The collecting of scientific material is another aspect of natural history with which many men in service are concerned. In 1944 the Smithsonian Institution issued *A Field Collector's Manual in Natural History,*[27] which has been widely distributed and used. A group of American botanists have formed an informal organization aimed to encourage Army and Navy personnel to keep up or develop an interest in scientific subjects by collecting natural history specimens. Specialists in various groups are identifying material and corresponding with enthusiastic naturalists, and already some fine collections have been received. Besides the Smithsonian's collector's manual there have been issued by the Department of Botany of the University of Michigan several pamphlets in a series of *Instructions to Naturalists in the Armed Forces for Botanical Work.* Those issued to date are *Seaweeds and Freshwater Algae*[31] by W. R. Taylor (no. 1), *Fungi and Lichens*[19] by E. B. Mains (no. 2), *Mosses and Liverworts*[30] by W. C. Steere (no. 3), and *Wood-destroying Fungi*[6] by D. V. Baxter (no. 8). These appear as Supplements to Company D Newsletter. "Company D" was a group of AMG men in train-

ing at the University of Michigan, who were specially instructed and encouraged to maintain their contact with natural science by engaging in scientific collecting whenever opportunity permits. This program of collecting by servicemen is interinstitutional. Collectors are being invited to send their collections to the Smithsonian Institution, attention of the writer of this article, whence they are being distributed to various specialists for identification. Many botanists and institutions the country over are receiving scientific specimens directly from servicemen, but in some cases the men find specimens can be sent to a government institution more readily than to others. Some men are asking that their collections be held for their own study on their return to civilian life and resumption of their interrupted training. Professor H. H. Bartlett of the University of Michigan, one of the sponsors of this interinstitutional program, has described it in an article *Gathering Scientific Materials—How Alumni Can Help*[5] in the *University of Michigan Official Publication* sent to all alumni in service. A similar article by him will soon appear in *Chronica Botanica.* The writer issued an article in 1944, *Plant Collecting by the Armed Services,*[35] which went to all medical officers in the army, and was republished for "Seabees." Hence, the invitation to collect scientific plant material has reached many men in practically all branches of the service. A private's wife reports that her husband's letters improved greatly in interest when he responded to the invitation to collect plants in New Guinea, and that he is looking forward to a lifetime of botanical exploration.

It is important to keep alive and develop the scientific interests of our young men while they are in service, in order to offset the danger that our country may emerge from this conflagration with world-wide obligations and opportunities for leadership in science, but without the properly qualified men needed to assume leadership in the geographical aspects of botany and zoology. The currently prepared literature mentioned in this article will go a long way toward assisting and maintaining the interest of our potential scientific leaders now in service and will give them essential training in observing and collecting material, which will at the same time increase the collections on which our leadership in natural science must be based.

WORKS MENTIONED AND WHERE THEY MAY BE OBTAINED*

1. **Allied Geographical Section, Southwest Pacific Area**
Special study of timber resources of the Philippine Islands. 113 pp. illus. map. Sept. 1944.
Mostly concerned with lumbering, but contains general information on forest types and individual trees with a 6-color folded vegetation map. A "card sorting key to the identity, properties and uses" accompanies this work with a supplementary pamphlet of directions. This is a "punch card" method for identifying timbers. This is a restricted publication prepared in Australia and not yet available for distribution.

2. **Allied Geographic Section, Southwest Pacific Area**
Vegetation study of the Philippine Islands. 120 pp. illus. map. 1944.
Contains much valuable botanical information. The colored map is the same as in the preceding work. Restricted and not yet available for distribution.

3. **Anonymous**
Flora and fauna of the Netherlands East Indies. 23 pp. 1944.
A general description. A mimeographed revision of part of the *Handbook of Netherlands East Indies* (1930). Issued by the Netherlands Information Bureau, Dupont Circle, Washington, D. C. in their Weekly Survey, no. 28.

4. **Arctic, Desert, Tropic Branch of the Army Air Forces Tactical Center**
Survival: A selected bibliography. 21 pp. Jan. 1945.
A source of information on the more important publications and their availability. "Distribution restricted to organizations or individuals working in cooperation with the armed forces, hence not generally available to the civilian public." (From an official letter.)

5. **Bartlett, H. H.**
Gathering scientific materials—how alumni can help. Univ. Mich. Official Publ. 46 (107): 10–15. April, 1945.
A description of the informal serviceman's collecting program. Obtainable without cost from the Bureau of Alumni Relations, U. of Michigan, Ann Arbor, Mich.

6. **Baxter, D. V.**
The collecting of wood-destroying fungi. Instructions to naturalists in the armed forces for

botanical field work. No. 8. Suppl. Co. D Newsletter, 23 pp. illus. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.
Free to servicemen.

7. **Blackman, Thomas M.**
Birds of the Central Pacific Ocean; a popular account of the sea-birds and shore-birds of the central area of the Pacific Ocean. 70 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.00.

8. **Carter, T. D., and G. H. H. Tate**
Animals of the Pacific world. 227 pp. illus. Macmillan Co., New York. 1945. $3.00.†
A popular account of the principal mammals.

9. **Cochran, Doris M.**
Poisonous reptiles of the world: A wartime handbook. Smithsonian War Background Studies, no. 10, 37 pp. illus. Mar. 1943.
Obtainable from the Smithsonian Institution, Washington 25, D. C. $.25, free to servicemen.

10. **Collins, H. B., Jr., A. H. Clark, and E. H. Walker**
The Aleutian Islands: Their people and natural history (with keys for the identification of the birds and plants). Smithsonian War Background Studies, no. 21, 131 pp. illus. map. Feb. 1945.
Prepared primarily to interest the servicemen in their environment. For availability see preceding entry. $.25, free to servicemen.

11. **Curran, C. H.**
Insects of the Pacific world. 331 pp. illus. Macmillan Co., New York. Not yet available. $3.75¶†
A popular account, probably available in October.

12. **Ethnographic Board and Staff of the Smithsonian Institution**
Survival on land and sea. 187 pp. illus. 1943; revised 1944.
An emergency manual with much botanical information, distributed widely to service personnel. Available to servicemen on application to the Division of Naval Intelligence, U. S. Navy, Washington 25, D. C., and in limited quantities to nonprofit institutions, such as colleges, schools, and libraries.

13. **Friedmann, H.**
The natural history of camouflage. Smithsonian Institution War Background Studies, no. 5, 17 pp. illus. 1942.
Concerns concealment in nature and the principles applicable in warfare. Obtainable from the Smithsonian Institution, Washington 25, D. C. $.25, free to servicemen.

14. **Keesing, F. M.**
Peoples of the Pacific world. 144 pp. illus. maps. Macmillan Co., New York. 1945. $3.00.†
A general account with incidental natural history references.

15. **Kramer, J. H.**
Native woods for construction purposes in the western Pacific region. (Preliminary edition, covering the Solomon Islands, Papua, northeast New Guinea, and the Bismarck Archipelago.) 197 pp. illus. map. May, 1944; (Revised edition covering the Solomon Islands, New Guinea Island, the Bismarck Archipelago, the Molucca Islands, Celebes, and the Philippine Islands.) 384 pp. illus. 5 folded maps. Sept. 1944.
A manual for identifying the principal commercial species. Obtainable by or for navy personnel without cost from the Bureau of Yards & Docks, Department of the Navy, Washington 25, D. C. For army personnel write to A. G. O. Publications Division, Executive Office, Room 2–C–966 Pentagon Building, Washington 25, D. C. Attention of Lt. T. Zbin. Civilians can now obtain it from the Superintendent of Public Documents, Government Printing Office, Washington 25, D. C., for $1.50.

16. **Kramer, J. H.**
Native woods for construction purposes in the South China Sea region . . . Burma, Malay Peninsula, Thailand, Sumatra, Java, Borneo, French Indo-China, and southeast China, including Hainan and Formosa. 277 pp. illus. map. 1945.
For nature of contents and availability see preceding entry. Price to civilians $.75.

17. **Lam, H. J.**
Fragmenta Papuana [Observations of a naturalist in Netherlands New Guinea]. Sargentia, no. 5, 196 pp. illus. 1945. $3.00.
Accounts of the author's travels with descriptions of the vegetation, plants, peoples, etc. and a bibliography. An English translation by L. M. Perry from the Dutch of a series of articles published in Dutch serials, 1927–29. Obtainable from the Arnold Arboretum of Harvard University, Jamaica Plain, Mass.

18. **Loveridge, A.**
Reptiles of the Pacific world. 272 pp. illus. Macmillan Co., New York. Not yet available. $3.00¶†
A popular account, probably available in October. The Infantry Journal edition, 243 pp. illus., is already available.

19. **Mains, E. B.**
The collecting of fungi and lichens. Instructions to naturalists in the armed forces for botanical field work. No. 2. Suppl. Co. D Newsletter, 8 pp. illus. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1944.
Free to servicemen.

20. **Mayr, Ernst**
Birds of the southwest Pacific; a field guide to the birds of the area between Samoa, New Caledonia, and Micronesia. 316 pp. illus. Macmillan Co., New York. 1945. $3.75.

21. **Merrill, E. D.**
Emergency food plants and poisonous plants of the islands of the Pacific. War Dept., Technical Manual, 10–420, 149 pp. illus. 1943.
Intended as a pocket manual. Contains illustrations and brief descriptions of the principal species with numerous vernacular names. Obtainable

from the Superintendent of Public Documents, Government Printing Office, Washington 25, D. C. $.15.

22. **Merrill, E. D.**
Plant life of the Pacific world. 336 pp. illus. Macmillan Co., New York. Not yet available. $3.50?†
An account of the vegetation and principal species, probably available in October. The Infantry Journal edition is already available.

23. **Mohr, E. C. J.**
The soils of equatorial regions with special reference to the Netherlands East Indies. 766 pp. illus. maps. Edwards Brothers, Ann Arbor, Mich. $7.50.
An English translation by R. L. Pendleton from the Dutch of *De Bodem der Tropen in het Algemeen, en die van Nederlandsch-Indië in het Bijzonder,* duplicated from typescript. The Netherlands Information Bureau, Dupont Circle, Washington, D. C. has reproduced the chapter "Climate and soil in the Netherlands Indies," 7 pp. [1944].

24. **Munro, G. C.**
Birds of Hawaii. 189 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.00.
A popular manual.

25. **Nichols, J. T., and P. Bartsch**
Fishes and shells of the Pacific world. 185 pp. illus. Macmillan Co., New York. Not yet available. $3.00?†
A popular account, probably available in October.

26. **Osborn, Fairfield** (editor)
The Pacific world: Its vast distances, its lands and the life upon them, and its peoples. 164 pp. illus. maps. W. H. Norton Co., New York. $3.00.†
A general account, largely geographical, with incidental references to natural history.

27. **Smithsonian Institution, Staff of**
A field collector's manual in natural history. 118 pp. illus. 1944.
Intended to assist servicemen in collecting animals of all sorts, plants, anthropological materials, fossils, rocks, etc. Obtainable from the Smithsonian Institution, Washington 25, D. C. $.50, free to servicemen.

28. **Sparhawk, W. N.**
Notes on forests and trees of the central and southwest Pacific area. 78 pp. Mimeographed. U. S. Forest Service. [1945].
Brief descriptions of the various island groups, with a bibliography. Obtainable from the author, U. S. Forest Service, U. S. Department of Agriculture, Washington 25, D. C. without cost.

29. **Sparhawk, W. N.**
Japan—forest resources, forest products, forest policy. 89 pp. Mimeographed. U. S. Forest Service. May, 1945.
Contains descriptions of forest regions and the principal trees with much information on forestry. For availability see preceding entry.

30. **Steere, W. C.**
The collecting of mosses and liverworts. Instructions to naturalists in the armed forces for botanical field work. No. 3. Suppl. Co. D Newsletter, 13 pp. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.
Free to servicemen.

31. **Taylor, W. R.**
The collecting of seaweeds and freshwater algae. Instructions to naturalists in the armed forces for botanical field work. No. 1. Suppl. Co. D Newsletter, 17 pp. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.
Free to servicemen.

32. **Tinker, S. W.**
Hawaiian fishes: A handbook of the fishes found among the islands of the central Pacific Ocean. 404 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.50.
An excellent guide.

33. **[U. S. National Museum, Staff of]**
[Natural history of the southwest Pacific.] 1944.
This is an informal, unnumbered, mimeographed series of circulars with titles as follows:
Birds of the southwest Pacific. 6 pp.
Birds of the western Pacific (Philippines, Formosa, and Ryukyu Islands). 7 pp.
Plants of the southwest Pacific. 9 pp.
Butterflies of the southwest Pacific. 6 pp.
Mollusks of the southwest Pacific. 4 pp.
Reptiles and amphibians of the southwest Pacific Islands. 8 pp.
Some common fishes of Australia and the South Pacific Islands. 7 pp.
Southwest Pacific starfishes, sea urchins and their relatives. 6 pp.
Each is a general account. Available on request to the U. S. National Museum, Washington 25, D. C. without cost.

34. **Van Schaack, USNR, Lt. G. B.**
Flowers of Island X. 38 pp. illus. 1945.
Descriptions and notes intended to supplement the botanical list and keys in "The Aleutian Islands: Their peoples and natural history." (See entry 10.) Published by the Welfare and Recreation Dept. [U. S. Navy?]. Probably not generally available.

35. **Walker, E. H .**
Plant collecting by the armed forces. Bull. Army Med. Dept. 85: 55–56. Dec. 1944; republished in Construction Battalion Activities 2 (3): 2–3. Mar. 1945.
A general account aimed to promote scientific collecting. Reprints obtainable from the author, Smithsonian Institution, Washington 25, D. C. without cost.

* Most of the government publications were intended for official use, hence stocks are limited. However, most of these are generously made available to the public as long as the supply lasts. Mention of restricted items 1, 2, and 4 has been approved by the Bureau of Public Relations, War Department.

† Special editions of these works may be obtained by or for servicemen only from The Infantry Journal, 1115 17th St., N. W., Washington, D. C. for $.25 each.

RELATIVE HEATING REQUIREMENTS FOR AMERICAN HOMES

By STEPHEN S. VISHER
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THE recent shortage of fuel has increased the interest in fuel requirements of all people whose homes are heated in the winter. Millions of families have wondered whether enough fuel could be obtained to carry them through the cold weather.

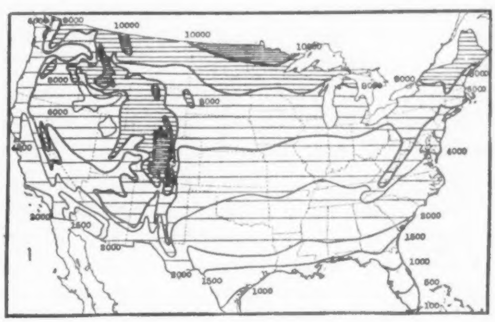
The amount of fuel required for adequate home heating depends, of course, upon several factors beside outdoor temperatures. One of these is the wind. However, since outdoor temperature is a major factor, more adequate information about what temperatures may reasonably be expected is increasingly desired.

Certain data recently released by the U. S. Weather Bureau have permitted the construction of the accompanying original maps. These data were compiled by, or under the direction of, J. B. Kincer, long chief of the Climate and Crop Weather Division of the Weather Bureau, in the few years before his recent retirement. They were made available at his special request in the hope that they would become more widely useful.

These data deal with a special sort of temperature description called a "degree-day," which is defined as a day having an average temperature one degree below 65° F. Thus all days with temperatures of 65° or higher are ignored in this discussion, on the reasonable assumption that house heating is not needed when the average daily temperature is 65°, for on such days the daytime average temperature normally is somewhat above 70° and the nighttime minimum not much below 60°. On days when the average temperature is 55°, ten degree-day units are registered; a day when the average temperature is zero has 65 degree-day units.

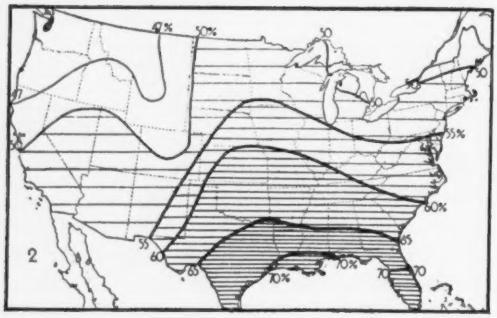
Data on the average number of degree-day units occurring at each of 208 "first order" Weather Bureau Stations by months permitted the construction of Maps 1–5. The variation from year to year in the number of degree-day units is shown, by sections of the country, in the graphs of Figure 1, prepared by Mr. Kincer.

Map 1, of the average annual number of degree-day units, shows that the range is from 100 in extreme southern Florida to 10,000 in northern North Dakota. Thus there is a hundredfold range. In the densely populated part of the country the average number of degree-day units lies mostly between 4,500 and 6,500; for example, it is 4,500 in the District of Columbia, 5,400 in New York City, 5,900 in Boston, 6,800 in Buffalo, 6,200 in Cleveland, 6,300 in Chicago, 4,600 in St. Louis, and 8,000 in Minneapolis. On the Pacific Coast the average is

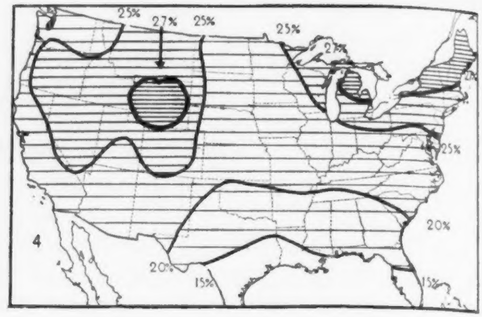


MAP 1. THROUGH THE YEAR
THE AVERAGE ANNUAL NUMBER OF DEGREE-DAY UNITS.

1,400 in Los Angeles, 3,100 in San Francisco, and 4,900 in Seattle. These wide contrasts imply a vast difference in fuel requirements. For example, Philadelphia has more than twice the degree-day units that Birmingham or Atlanta has, Milwaukee has one-eighth more than Chicago, and Cleveland has one-fourth more than Cincinnati. These percentage variations do not mean quite so great a difference in the fuel supplies needed, however, because of differences in insulation of houses, for example, but they reveal an important range in an appreciable item of the cost of living.



MAP 2. IN WINTER
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.



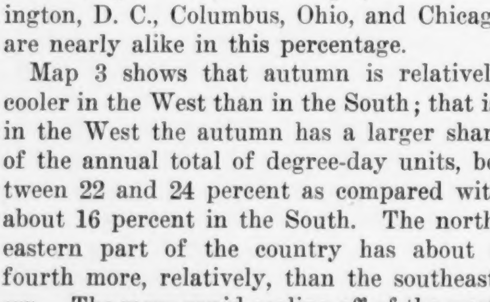
MAP 4. IN SPRING
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.

Map 2 shows the fraction of the degree-day units which occur during the winter (December–February). It reveals that winter has about 70 percent of such units in the Deep South but that the Northwest and extreme Northeast have less than half of their cold weather units in winter. For most of the people of the United States, the three winter months require only slightly more than half of the annual fuel needs. It is interesting to note, for example, that Washington, D. C., Columbus, Ohio, and Chicago are nearly alike in this percentage.

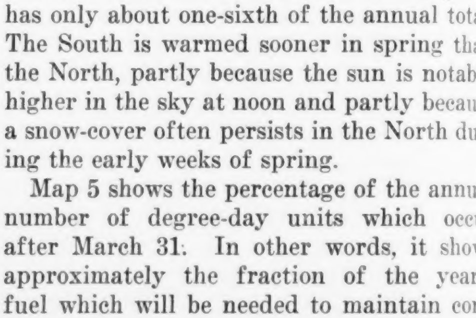
Map 3 shows that autumn is relatively cooler in the West than in the South; that is, in the West the autumn has a larger share of the annual total of degree-day units, between 22 and 24 percent as compared with about 16 percent in the South. The northeastern part of the country has about a fourth more, relatively, than the southeastern. The more rapid cooling off of the western third of the country is associated with its higher elevation above sea level and its greater dryness. In the humid East the considerable contrast between North and South, despite similarity of elevation and humidity, is largely associated with the cooling effects of the snowfall of the North. (The South seldom receives snow in the autumn while in the northern part of the North snow is general in November.)

Map 4 shows that the spring quarter year in much of the northern half of the country has about a quarter of the year's degree-day units, but near the Gulf of Mexico the spring has only about one-sixth of the annual total. The South is warmed sooner in spring than the North, partly because the sun is notably higher in the sky at noon and partly because a snow-cover often persists in the North during the early weeks of spring.

Map 5 shows the percentage of the annual number of degree-day units which occur after March 31. In other words, it shows approximately the fraction of the year's fuel which will be needed to maintain comfortable indoor temperatures after April 1. The variation in the more heavily populated parts of the country is less than might be



MAP 3. IN AUTUMN
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.



MAP 5. AFTER MARCH 31
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.

expected; most housekeepers need approximately one-eighth of their house-heating fuel supply after April 1. In Washington, D. C., the percentage is 10, in New York City 12, in Chicago 12, and in Los Angeles and San Francisco also about 12.

Three additional maps, not here printed, may be briefly described. The fraction of the annual total of degree-day units which occur before November 1 range from about 3 percent in the Deep South to 7 to 10 percent in the Northeast and 11 to 13 percent in the Northwest.

Half of the annual total of degree-day units normally have occurred by January 15 in the Deep South, by January 18 in most of the western half of the country, and also in the middle South, but not until January 24 in the Northeast. In other words, midwinter occurs, so far as degree-day units are concerned, on January 13 in Louisiana and New Mexico, on January 15 in Florida, South Carolina, and Arkansas, on January 17 in most of the western states and in Tennessee, and on January 20 in Maryland, Ohio, and northern Illinois. The midwinter date comes latest in New England, Michigan, and New Jersey where January 24 marks the accumulation of 50 percent of the total seasonal degree-day units.

Throughout the southeastern third of the country the summer quarter year normally has no degree-day units. However, about one percent of the annual total of such units occur along a line extending from New Jersey to Iowa and thence southwest to New Mexico. About 3 percent of the year's total occurs in summer in New England, Michigan, and North Dakota, and 4 or 5 percent in the Northwest, from Colorado to Oregon and Washington.

Fuel requirements vary not only from place to place but also from year to year. Although all parts of the United States experience variations in amount of cold, a careful study of the departures from the normal, or average, conditions from 1898 to 1942 shows considerable regional contrast in variability. The variation is least in the Northeast (New England and New York) where only 5 years of the 42 studied departed as much as 12 percent from average, and the greatest departure was only 14 percent. The South Atlantic States, by contrast, had a greater than 15 percent departure from average or normal; two seasons were more than 30 percent colder than normal, and 1930–1931 was 39 percent warmer than normal. Of the 10 regions of the United States studied by J. B. Kincer, the average percentage variation from normal was approximately as follows (estimated from his graphs by years): Northeast 10.0; East Northcentral States 11.8; Northern Interior (North Dakota and adjacent states) 12.3; Central Interior (Kansas, etc.) 12.3; Rocky Mountain States 13.0; North Pacific States 15.3; California 23.4; Middle Atlantic States 12.9; South Atlantic States 28.6; Southcentral States 22.1. These data show that the average range along the southern border of the country is more than twice as great as along the northern border.

The extreme variation experienced in these 42 years was somewhat more than twice as great as the average variation. It was least in the Northeast (26 percent from normal, 14 above, 12 below); it was 30 percent in the Northern Interior, about 32 per cent in the belt that extends from New Jersey to Kansas and from Kentucky to the Great Lakes; on the Pacific Coast the extreme range at the North averaged 41 percent, while in California it averaged 60 percent. The increase in range southward from Virginia and Oklahoma is sharp, an extreme range of 60 percent occurring in South Carolina and central Texas and one of more than 70 percent near the Gulf Coast.

Thus, although the winters of the North average much colder than those of the South, a long-time average reveals that the cold of the North is more regular and predictable than that of the South.

A climatic problem of widespread interest —how our climate is changing—is illuminated by the Kincer data on the departures from the normal of the averages of annual degree-day units. Kincer's studies reveal that the last half of the period 1898–1942 has been warmer than the first half in all regions of the United States. The warming has been greatest in California; next comes the Southeast, despite two exceptionally cold seasons. The Northeast has experienced the least change toward warmer weather.

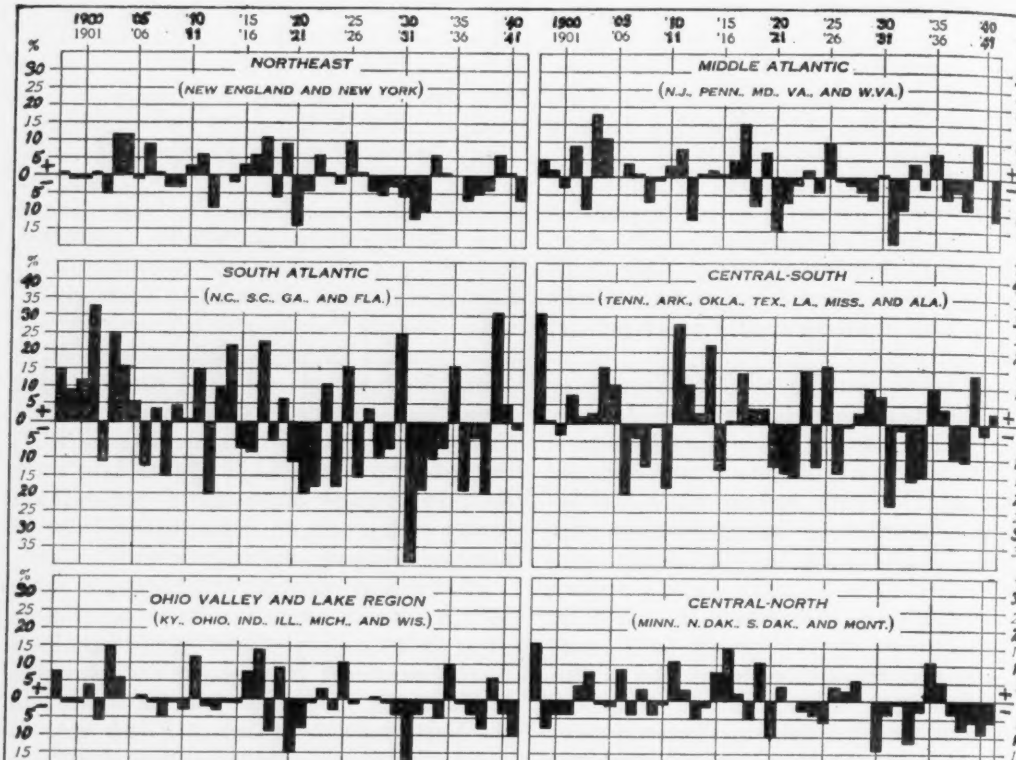


FIG. 1. DEPARTURE FROM NORMAL OF DEGREE-DAY UNITS

BAR GRAPHS BY J. B. KINCER OF THE U. S. WEATHER BUREAU, SHOWING PERCENTAGE DEVIATIONS, 1898–1942.

Figure 1 shows detailed bar graphs for each of the years (centering in winter) 1898–1942 for each of the 10 regions studied by Kincer. The downward bars indicate fewer than the normal number of degree-day units, and hence fewer cool or cold days.

SCIENCE ON THE MARCH

THE BERLIN CONFERENCE

The Report of the Berlin Conference, released on August 2, 1945, is more than a political document. It obviously reaches into every phase of German life and thought, and it affects the Reich's geography, technology, industry, and economy. What is less obvious but no less true is the fact that the document as approved by J. V. Stalin, Harry S. Truman, and C. R. Attlee is a blueprint for the future of central and southern Europe. From this standpoint some of the provisions of especial interest to scientists invite comment.

Provision is made for the control of "all German public or private scientific bodies, research and experimental laboratories, et cetera, connected with economic activities." Scientists will immediately think of German contributions in the fields of medicine, psychology, chemistry, and all the other sciences, pure and applied. They will not forget the commercialization of nitrogen fixation which served both as a boon to agriculture and as a scourge to man when the process was turned to the manufacture of explosives in 1914. They will recall the development of metallic magnesium, which gave industry its lightest metal but likewise burned much of London in the fire raids of 1940 and 1941. They will remember hydrogenation and the correlative processes whereby Germany's lignite was converted into petroleum products and synthetic rubber—basic raw materials for peacetime industry, but at the same time the means of motorizing the German military machine and of bringing the *blitz* to Poland, Denmark, Norway, Holland, Belgium, France, Yugoslavia, Greece, Russia. They may wonder whether the stipulated controls will smother inventive genius, as well as the Prussian penchant for war.

The Conference Report decrees the elimination of Germany's war potential, specifically by restricting the "production of metals, chemicals, machinery and other items . . . to approved postwar needs," and by the removal of "productive capacity not needed for permitted production . . . in accordance with the reparations plan. . . ." It is further stipulated that "the German economy shall be decentralized for the purpose of eliminating the present excessive concentration of economic power;" that "primary emphasis shall be given to the development of agriculture and peaceful domestic industries;" and that "measures shall be taken promptly to enlarge coal production."

The terms of the document prepared at the Conference leave no doubt about responsibility for the holocaust which the Germans brought upon the civilized world in 1939, or about the utter defeat which they have deservedly suffered. But the provisions suggest that preoccupation with Germany's war potential may have led inadvertently to the adoption of measures which, while chaining the criminal, will also cripple his unfortunate victims.

Germany's prewar economy was adjusted to a population of 60,000,000 in Germany proper and of 80,000,000 in "Greater Germany." Despite its northerly latitude, its mediocre to poor glacial soils, and its extensive upland terrain, Germany cultivated intensively every available acre of its 181,000 square miles, leading the world in crop yields per acre. Soils and climate compelled her to concentrate on root crops, of which potatoes and beets are outstanding, and upon the lesser grains—rye, barley, and oats. It will be difficult, and perhaps impossible, to increase agricultural production beyond the point reached by the Germans themselves; yet peak agricultural production was never sufficient to provide either a livelihood or food for the entire population. The best the Council of Foreign Ministers or its agents can hope for in administering the agricultural provisions of the Conference Report is the restoration of German farm production to the maximum achieved in the decade of the thirties.

The Conference Report also stresses the mining of coal, which is the Reich's greatest natural resource. A question inevitably follows: To what use will the coal be put? The Report provides one answer—"peaceful

domestic industries." But such an answer demands analysis. As war broke out, German coal production reached an all-time high of 200,000,000 metric tons of bituminous coal and 230,000,000 metric tons of lignite. Roughly one-third of the bituminous coal came from Silesia, which has been awarded to Poland. Saar tonnage was relatively small, and the quality of Saar coal placed severe restrictions upon its use for industrial purposes. The Ruhr yielded approximately two-thirds of the gross production and a still higher proportion of the high-grade industrial fuel.

In the January issue of THE SCIENTIFIC MONTHLY Chauncy D. Harris pointed out that the coal mines of the Ruhr have supplied continental Europe, except the U. S. S. R., with half of its energy requirements. He concluded that, "barring unforeseen discoveries, the Ruhr district is likely to remain the most important European source of power."

It may be planned to distribute Ruhr coal among Germany's neighbors to rehabilitate industries that were crippled by the military regime and war economy imposed upon these nations by the Germans. But the arbitrary destruction of installed industrial capacity which draws directly on local supplies of high-quality fuel is a dubious expedient, particularly when equivalent capacity is not available elsewhere to produce the iron, steel, and other durable goods which central Europe so sorely needs. Not only does such a policy flout sound economic practice, but it seriously threatens effective economic recovery within the entire central and southern European trading area, with a gross population of nearly 200,000,000.

Germany's lignite, or brown coal, has been skillfully utilized in steam plants for the generation of power and as raw material for the manufacture of oil, rubber, and other synthetic products. The power generated, supplemented by modest supplies of hydroelectric energy, has operated reduction plants for aluminum, magnesium, and other metals, as well as factories for the manufacture of textiles and similar peacetime products. Available power from lignite far exceeds the requirements of the strictly "peaceful domestic industries." The physical properties of lignite are such as to preclude shipment—it must be used at the mines or not at all. Hence curtailment of industrial use means a drastic reduction of output and a substantial net loss in the power potential of central Europe. It has been proposed to generate the power and to transmit the energy to surrounding countries. This proposal, however, blandly ignores the fact that Germany's immediate neighbors, which lie within economic transmission distance, lack the industrial plants to use the power and the skilled labor to apply it even if the plants were built.

The peoples of the United Nations are in no mood to worry about a balanced economy for Germany or about employment for 60,000,000 Germans. But the Report of the Berlin Conference suggests that the interdependence of several national economies is being overlooked. French and Swedish iron ore; French and Yugoslav bauxite; Danubian, Dutch, and Swiss foodstuffs; Baltic lumber and many another raw material were traded for German machinery and for manufactured materials which these countries cannot make from native raw materials with native labor. The industrial economy of continental Europe exclusive of Russia is a delicate web, of which the Ruhr is the only available center. For the sake of the 135,000,000 non-German peoples involved in the web, the center must be preserved, even though the 60,000,000 Germans will necessarily benefit. For this reason, retention of industrial capacity with rigid controls appears to offer a sounder and less hazardous long-range program than the restriction, removal, decentralization, and elimination prescribed at the Berlin Conference.—HOWARD A. MEYERHOFF.

BOOK REVIEWS

A SCIENTIST'S EXPERIENCE IN MEDICAL RESEARCH

The Way of an Investigator. Walter B. Cannon, M.D. 229 pp. 1945. $3.00. W. W. Norton and Company, New York.

Colleagues who have followed Dr. Cannon's outstanding work in medical research, medical education, and in other public service, in peace and in war, for 50 years, will expect something more than a traditional autobiography in this volume. This they will find in most of the chapters of *The Way of an Investigator.* The first chapter gives us a glimpse of Dr. Cannon's ancestry (French-Canadian and Scotch-Irish), his boyhood days in Wisconsin and Minnesota, and his student adventures at Harvard University and the Harvard Medical School 50 years ago. He grew up close to the soil and was conditioned early to the necessity and the dignity of labor. Dr. Cannon may be said to be, through the Harvard Professor of Physiology, Dr. Bowditch, the grandson of the great physiologist, Ludwig, of Leipzig.

The meat of the narrative, the timeless, simple and clear story of Dr. Cannon at work in the research laboratory, in the lecture halls, in scientific assemblies at home and abroad, starts in the second chapter (p. 22) and covers most of the book up to p. 174. Dr. Cannon pioneered in many fields of fundamental biology. The story is clear, even to laymen, and there is little or no bypassing of the author's own errors in experiment or in interpretation. Pointed, and at times humorous, illustrations and quotations from great men of science of the past illuminate and enliven the pages. These chapters should be *must-reading for all medical students* if not all students in science in college and high school. Here Dr. Cannon will be teaching by a great example (I hope) for another 50 years. Not the least of these teachings by example is Dr. Cannon's deeds and words touching our fellow men in other lands, that is, Dr. Cannon's international experience and outlook.

As evidence of the directness, clarity and simplicity of style, the essential impersonality and universality of *The Way of An Investigator,* I quote the opening paragraph of the second chapter (p. 22):

> Investigators do not march straight to their goal with ease and directness. In their imagination they see a possible fact and they set forth to learn whether their foresight can be realized. Or they come upon something which is puzzling and challenging and which they wish to explain; then they try in various ways to relate it to other phenomena that would solve the riddle. Obstacles and difficulties are sure to be encountered. The search for understanding is an adventure or, more commonly, a series of adventures. If an attempt in one direction fails, the failure is not discouraging to an eager explorer. There are other possible approaches to the end in view and relentlessly, one after another, these are tried. When the goal is reached, there is occasion for joy and exultation. A conquest has been achieved. New knowledge has been gained which deeply satisfies both the explorer's adventurous spirit and his persistent curiosity.

Dr. Cannon was President of the American Association for the Advancement of Science in 1939. The Association has (I think wisely) selected *The Way of An Investigator* as one in its nontechnical science series. This volume is not above the comprehension of laymen, and wide reading of Dr. Cannon's story would broaden the base of real understanding of science in our beloved land.

A. J. Carlson

Department of Physiology
University of Chicago

BOTANY AND AGRICULTURE IN LATIN AMERICA

Plants and Plant Science in Latin America. Edited by Frans Verdoorn. 384 pp. Illus. 1945. $6.00. Chronica Botanica Company, Waltham, Mass.

This interesting volume is No. 16 of *A New Series of Plant Science Books* being issued by the Chronica Botanica Company. One's first impression is of its comprehensiveness, and he regrets that in this volume, as in its associates, the type is too small to attract the general reader. For, while many chapters are specialized, there is much here that should receive wide attention. Again, the confused sequence of themes will be discouraging to some, but the consultant is directed to the "Complete and Detailed Table of Contents" at the rear which gives a topical analysis of each paper. The sub-

jects considered are so diverse as to defy easy grouping.

Plants and Plant Science in Latin America contains papers prepared by as many as 85 authors. Some dissertations are of general scope geographically, while others are devoted to particular countries. Among the many themes discussed, most pertain to botany and its applied phase, agriculture. Dr. Verdoorn has coordinated the whole, and written a stimulating "Introductory Essay." To it and its message we shall return after attempting to show in more detail what the whole work comprises.

Much of the book recalls the *Naturalist's Guide to the Americas*[1] of nearly twenty years ago. To that some 125 authors contributed, and both works show a resultant inequality in the value of the individual sketches. Only the smaller portion of the *Naturalist's Guide* was devoted to Latin America, and then south only to the equator; *Plants and Plant Science* covers all territory from the southern boundary of the United States and from the West Indies to Cape Horn. The *Naturalist's Guide* considered both plants and animals, but these only as they occur in nature; the later work plants only, but these both in nature and in cultivation. A main feature of both is the often admirable sketches of the vegetation (or, for the earlier work, the biota) of particular countries. These are quite comparable and often supplemental in the two works, sometimes with a fuller and clearer summary in the one and sometimes in the other. (A surprising comment is how rarely these sketches in the later make any reference to those in the earlier work.) But the later work carries much that the earlier lacked.

Such are the series of general sketches, discussing certain themes for Latin America as a whole. Of most value to naturalists will be the "Phytogeographic Sketch of Latin America," and there are summaries of the climatology and mineralogy, of the ethnobotany, of the history of botanical exploration, and of such practical subjects as food aspects and soil conservation.

[1] Naturalist's Guide to the Americas. Edited by Victor E. Shelford. Illus. 761 pp. 1926. Williams and Wilkins Co.

It is in its trend toward applied science that the later differs most strongly from the earlier work. Its first contribution essay is on "Some Problems of Tropical American Agriculture," which is followed (but not immediately) by a comprehensive summary of the "Principal Economic Plants of Tropical America." For many of the countries there are special articles on the "Plant Resources," while not geographically limited are articles on "The Production of Essential Oils in Latin America," "Notes on Cinchona Culture," and an especially detailed account of "Hevea Rubber Culture in Latin America, Problems and Procedures."

The progress of pure science also holds its place in such themes as "Plant Pathology in Latin America," "Paleobotanical Work in Latin America," "Plant Breeding, Genetics and Cytology in Latin America," and "The Location of Botanical Collections from Central and South America."

Topics are far from exhausted, but one must pass from such listing to see what the editor himself has put into the volume. Even though he has taken credit for none of the special discussions within the body of the book, his has been no mean contribution. One senses Dr. Verdoorn's historical interest in the attractive illustrations reproduced from various works of travel, as well as in "A Selected List of Travel Books of Botanical Interest," further bibliographic lists, a list of plant science institutions and societies, etc. His most significant contribution is his introductory essay, entitled "The Plant Scientist in the World's Turmoils." Its message is timely, and gives his reason for undertaking the present volume.

Dr. Verdoorn is a true internationalist. Readers will recall that he commenced his *Chronica Botanica* on such a basis in Holland, and, because of the increasing exigencies there, moved the enterprise before this World War to this country. In the present essay he discusses the growth of nationalism over the World since the first World War and shows how it has superseded the actual move toward internationalism that had been gaining headway in the first decades of this century. He now pleads that science with its broad outlook should aid the latter all-essential movement. The incentive to the

present task has been the desire to further international understanding throughout the Western Hemisphere. Perhaps by such regional understanding and cooperation can the internationalism of the future be best developed. A step lies in such undertakings as the present, for not only does this volume include many countries in its view, but the contributing authors are from various Latin-American as well as Anglo-American countries, and they have written in English, Spanish, Portuguese, and French.

In opening his introductory essay, Dr. Verdoorn quotes from Carl Lumholtz' *Unknown Mexico*, published in 1902, and this reviewer is glad to pass on the selection to his readers now: "It is unnatural to be without a special love of the country of one's birth, just as a man has more affection for his family than for other families. But let our allegiance extend to the whole globe on which we travel through the universe, and let us try to serve mankind rather than our country right or wrong."

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MATHEMATICAL METHODICITY

How To Solve It. George Polya. 204 pp. Illus. 1945. $2.50. Princeton University Press, Princeton, N. J.

THE author of this little volume is a distinguished professor of mathematics at Stanford University, formerly dean of the school of mathematics and physics at the Federal Institute of Technology in Zurich. In this book he presents in very simple and detailed form an analysis of steps to be taken in solving problems. The illustrations are mainly from the field of mathematics, and the writer had this field principally in mind, but it is to be noted that the range of applications is far wider—to problems in general.

In brief summary, he states:

First. You have to *understand* the problem.
Second. Find the connection between the data and the unknown. You may be obliged to consider auxiliary problems if an immediate connection cannot be found. You should obtain eventually a *plan* of the solution.
Third. *Carry out* your plan.
Fourth. *Examine* the solution obtained.

He then analyzes in some detail how these steps are to be taken, and how a teacher may unobtrusively aid the student in carrying out the program. Illustrations from the domain of anagrams, puzzles, elementary geometry, algebra, elementary calculus, and number theory, carried out in part as a dialogue between student and teacher, make the analysis concrete.

Most of the book is devoted to Part III, "Short Dictionary of Heuristic," which contains sixty-four articles on alphabetically arranged topics, from "Analogy," "Auxiliary Elements," and so on, to "Working Backwards." To the professional mathematician this presents a lively summary and analysis of methods which he habitually uses in his research work, couched in everyday language. To others there may well be here suggestions and illustrations of methods which are unfamiliar or novel; which should be given careful consideration and tried in the solving of problems that otherwise appear untractable.

Though fundamentally serious, the book is lively, entertaining, and not without humor. We may relish the introductory paragraphs to certain topics in his Dictionary:

> *Rules of discovery.* The first rule of discovery is to have brains and good luck. The second rule of discovery is to sit tight and wait till you get a bright idea.
>
> *Rules of style.* The first rule of style is to have something to say. The second rule of style is to control yourself when, by chance, you have two things to say; say first one, then the other, not both at the same time.
>
> *Rules of teaching.* The first rule of teaching is to know what you are supposed to teach. The second rule of teaching is to know a little more than what you are supposed to teach.
>
> *The traditional mathematics professor* of the popular legend is absentminded. He usually appears in public with a lost umbrella in each hand. He prefers to face the blackboard and to turn his back on the class. He writes *a*, he says *b*, he means *c*; but should be *d*. Some of his sayings are handed down from generation to generation.

"In order to solve this differential equation, you look at it till a solution occurs to you."

"This principle is so perfectly general that no particular application of it is possible."

"Geometry is the art of correct reasoning on incorrect figures."

"My method to overcome a difficulty is to go around it."

"What is the difference between method and device? A method is a device which you use twice."

While the book may properly be called a mathematics book, it is quite clear that it bears little resemblance to textbooks with which the reader is familiar. I recommend it highly to any person who is seriously interested in finding out methods of solving problems, and who does not object to being entertained while he does it.

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PLANNING A WORLD FOOD SUPPLY

Food for the World. Edited by Theodore W. Schultz. 353 pp. Illus. 1945. $3.50. University of Chicago Press.

DURING the past fifteen years considerable attention has been given to the existing paradox of widespread undernourishment and malnutrition in a world burdened by disturbing regional food surpluses. This problem and its implication for postwar food policies furnished the central theme of discussion at the Twentieth Institute of the Norman Wait Harris Memorial Foundation, held at Chicago, September 4–8, 1944.

Food for the World is a valuable record of the program of that Institute, which was planned to bring to focus upon the subject of public food policy the knowledge and adjudgment of leading students in the fields of nutrition, population, agricultural economics, and international relations. In addition to the 23 essays presented, the book contains an illuminating introduction by Theodore W. Schultz and a report of the interesting informal discussions that followed the six major groups of essays.

The reviewer knows of no other book from which a reader can get so complete and authoritative a summary of the important background factors of the world's food problem as is given here. If, however, one seeks and hopes to find close agreement among the participants as to appropriate national and international food policies, he is likely to be disappointed. Yet, even the discussions on food policy contain a significant core of agreement.

Part I of the book consists of essays by Frank G. Boudreau and John D. Black on the development of the international movement to raise levels of food consumption—a movement that first became important in the mid-thirties.

Part II, by Frank W. Notestein and Frank Lorimer, directs attention to the currently different rates of population growth in various countries and to the changes likely to be witnessed over the next few decades. In the densely settled and undernourished Orient, domestic food production shows no signs of being able to keep pace with the rapidly growing population. In the Western World, on the other hand, declining rates of population growth and continuing technical improvements tend to be reflected in chronic surpluses of farm products.

Part III, on Nutrition, is one of the highlights of the book. Here one finds a valuable survey of present knowledge in the field of nutrition, outlined by such authorities as C. A. Elvehjem, L. A. Maynard, Paul R. Cannon, Ancel Keys, and Lydia J. Roberts. These experts (especially Maynard) put needed emphasis on the unknowns, as well as the knowns, in this relatively new science. And the "government planner" should note that even within the known sector of the field, students of nutrition disagree as to whether "recommended dietary allowances" should be set at levels "only sufficient to prevent clinical or biochemical signs of deficiency" or (like the recommended allowances of the Food and Nutrition Board) at appreciably higher levels that insure "a fair degree of tissue stores."

The three remaining groups of essays—on Food Supplies, International Relations, and Consequences and Policy—are devoted primarily to economic facts and analysis. Among the contributing economists are many

well known for their work in the fields of agricultural economics and international relations: Theodore W. Schultz (Editor), Paul H. Appleby, Percy W. Bidwell, Karl Brandt, Allan G. B. Fisher, E. W. Gaumnitz, Edward S. Mason, Margaret G. Reid, Leroy D. Stinebower, Henry C. Taylor, Howard R. Tolley, Walter W. Wilcox, and P. Lamartine Yates.

Among these authorities there is close agreement in analysis of the world's food problems, but considerably less agreement with regard to proposals for policy solutions. It is therefore noteworthy that a substantial amount of accord is registered on the following important points bearing on food policies:

(1) Some measure of official direction of food production and distribution in the postwar period is inevitable and probably desirable.

(2) Any satisfactory agricultural and nutrition policy must include provision for the maintenance of high levels of national and world employment, for continued increase in economic output per man-hour, and for expansion of international trade.

(3) The pressure of population against food supplies in India, China, and similar areas cannot be relieved quickly and permanently through importation of foodstuffs from abroad. Solution of the food problems of these countries must be based upon improvements in domestic agriculture, industrialization, and mass education—developments that may be expected to raise the socio-economic status of the people and eventually reduce the rate of population growth.

(4) There is no easy solution for the agricultural surplus problems of the United States and other Western countries, though these problems would be minimized in an expanding world economy, under conditions of practically full employment and freer international trade.

(5) The agricultural price policy recently pursued by the United States has protected high-cost farmers, raised prices to consumers, postponed the shifting of agricultural resources from products in chronic surplus to foods needed to raise the nutritional status of the population, and curtailed the flow of American farm products to foreign markets.

These accepted ideas are probably more important than the expressed differences of opinion on policy, which, in any case, are too numerous to outline here.

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SCIENCE A LA MODE

Science Today and Tomorrow: 2d Series. Waldemar Kaempffert. 279 pp. 1945. $2.75. The Viking Press, New York.

Science Year Book of 1945. Edited by John D. Ratcliff. 224 pp. 1945. $2.50. Doubleday, Doran & Co., New York.

Careers in Science. Philip Pollack. 222 pp. Illus. 1945. $2.75. E. P. Dutton & Co., New York.

KEEPING abreast of science these days is no child's play, and the science writers who attempt to keep us well informed are to be encouraged. These three books, though not written with identical aims by any means, have this in common: they review current scientific progress for the layman and indicate a few of the opportunities in science and a little of the "shape of things to come."

Mr. Kaempffert's book is the most stimulating of the three. His 24 chapters cut a wide swath, and their appeal lies not only in the factual accounts of recent scientific discoveries and developments but also in the author's speculations for the future. It is easy enough to say that the world of tomorrow will be violently changed by science, but it takes a Kaempffert to visualize these changes, to dare to make rather imaginative but definite predictions, and to render them convincing to the reader who may be inclined to pooh-pooh this H. G. Wells sort of stuff. When we start reading about such possibilities for the future as rocketing-through-space-to-Mars, psychosurgery, teletaction, teleolfaction, telegustation, etc., etc., we instinctively exclaim, "Tell it to Sweeney!" But then, when we finish reading we become apothegmatic. *Today's romance is tomorrow's reality. Only dolts disbelieve in miracles. There are more things in heaven and earth, Horatio. . .*

Mr. Kaempffert believes in the efficacy of science as a world-unifying agency. "Science and technology," he says, "are the most potent influences in the world today," and they are "about the only unifying forces on which all men are agreed." In his final

chapter, "Through Science to World Unity," he pleads for a World Scientific Commission to organize and direct science toward this international unity and to educate the masses in the fundaments of science. We agree with him that the experiment is worth making, for if Science is to become king the people everywhere ought to begin knowing something about their new ruler. "It may take decades, even generations," says Kaempffert. It will, so far as America is concerned, unless our education is raised considerably above the present donald-duck level.

Science Year Book of 1945 is the fourth such anthology to be edited by Mr. Ratcliff. He has aimed to reprint the latest, most exciting, and most readable developments in the fields of medicine, physics and chemistry, aviation, and other sciences. The articles have appeared in such popular magazines as *Reader's Digest, Collier's, Saturday Evening Post,* and *Hygeia,* among others. Penicillin, blood derivatives, DDT, buzz bombs, electrons in medicine and industry, high-vacuum physics, and industrial research are featured. The selections are conspicuously of uneven scientific value, and it is hard to see how some of them make the grade. Several of the sciences, such as anthropology, botany, and zoology (except for an article on animal migration) are unrepresented, which indicates, I suppose, not that these branches have made no noteworthy progress since 1944 but that writers in these fields have not attained the level of popularization that Mr. Ratcliff's yearbooks require.

Careers in Science is written especially for young people who may be thinking seriously of entering scientific work as a livelihood. It is a book of vocational guidance and is of a series of "career" books. The plan throughout is to try to tell the young prospective scientist what he is getting into, what the opportunities are in his chosen field, what he may expect to earn, the directions that science is taking. Chemistry, physics, biology, and geology seem to offer the best prospects. Although rather indifferently written and although not all the author's facts and figures are strictly up to date (such as the names of and salaries offered by various United States Government agencies), the book contains much good advice and valuable information. One recurrent query is asked the would-be scientist: Have you got what it takes? Have you the price of admission? Besides Mr. Pollack's contribution, which was made in collaboration with Vocational Guidance Research, the book has an introduction by H. C. Madsen, of Westinghouse; a chapter on "The Outlook for the Physicist and Prospective Physicist in Industry," by Dr. Albert W. Hull, of General Electric; and a chapter on "How Can We Develop Inventors?" by Dr. Charles F. Kettering, of General Motors. The volume is abundantly illustrated and is complete with bibliography and index. This book should find its way into all our secondary-school libraries. If compulsory military training for our youth comes, science and technology will be the losers, and it therefore behooves science to look now to her training grounds and garnishee all the latent scientific talent anywhere in sight. This book may help a little.

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CRUSADING MOLDS

Microbial Antagonisms and Antibiotic Substances. Selman A. Waksman. 350 pp. Illus. 1945. $3.75. The Commonwealth Fund, New York.

THE advent of Professor Waksman's book is a very timely one. It has frequently happened in the history of science that, like a good gold strike in new territory, a comparatively simple observation or discovery has set off a mass trek into new fields. Such has been the situation for the past four years with respect to antibiotics, and it is interesting to look back and try to put one's finger on the "strike" which set it off. The conception of antibiosis is not at all new, Pasteur himself having commented on phenomena of this sort as much as seventy-five years ago. Neither is the idea of using antibiotic substances for therapeutic purposes a novel one, the Germans having experimented with pyocyanase and pyocyanine as much as fifty years ago. Moreover, in comparatively recent times, the discovery of the therapeutic activity of gramicidin did not serve to set off the deluge of antibiotic research with which

we are now confronted, and penicillin had already been known for twelve years without having attracted very much attention. In retrospect, it now appears that the spark which started the conflagration was the demonstration of the almost micraculous therapeutic properties of penicillin in the treatment of certain conditions for which there had previously been no effective remedy. The result has been a tremendous burst of activity to find new antibiotic substances, not only in microorganisms but in the lower and higher plants as well.

The author opens the subject of *Microbial Antagonisms and Antibiotic Substances* with a discussion of the association of mixed populations of microorganisms in water, soil, and composts. Although many of these relationships are symbiotic, those which are of an antibiotic nature have here been singled out for special attention. This provides the entree into the discussion of the general subject of the book, which is introduced with sections on methods for isolating antagonistic microorganisms and the measurement of the activities of their associated antibiotics. Following this there are separate chapters devoted to each of the various groups of organisms known to produce antibiotics; both the chemistry and biological properties of known compounds of this class are discussed insofar as it is possible. Finally, there are chapters dealing with the use of antibiotics in the control of animal and plant diseases.

Microbial Antagonisms and Antibiotic Substances is highly accurate, complete, and up-to-date in every respect, including a bibliography of more than a thousand references. However, no critical evaluation is given for most of these papers, and one cannot help wishing that the author, considering his pre-eminence in the field, had done something more than merely list them and allow the reader to decide for himself as to their relative importance. This book is in no sense a popular treatment of the subject; it is definitely a book for the scientific worker in this and closely allied fields, and as such can be very heartily recommended.

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DEEP SEA AND ANTARCTIC EXPLORATION REPORTS

The Genus Ceratium in the Pacific and North Atlantic Oceans. Scientific Results of Cruise VII of the Carnegie. Biology V. Herbert W. Graham and Natalia Bronikovsky. 209 pp. Illus. 1944. Paper bound $2.00; cloth bound $2.50. Carnegie Institute of Washington.

The Scientific Results of Cruise VII of the Carnegie. Oceanography II. *Part I. Marine Bottom Samples Collected in the Pacific Ocean.* Roger R. Revelle. *Part II. Radium Content of Ocean-Bottom Sediments.* Charles S. Piggot. 195 pp. Illus. 1944. Paper bound $2.00; cloth bound $2.50. Carnegie Institute of Washington.

Reports of Scientific Results of the United States Antarctic Service Expedition, 1939–1941. 398 pp. Illus. 1945. $4.00. Proceedings of the American Philosophical Society.

ALTHOUGH expeditions to the open ocean and the Antarctic were among the casualties of the war, publication of their results is being carried through in spite of the difficulties which accompany these unsettled times. To be sure, the last major oceanographic expedition, the seventh cruise of the *Carnegie,* ended with the destruction of that fine ship at Apia in 1929, but is was not until 1942 that publication of the results was begun. Expeditions since that time have not been so extensive and have usually been connected with some particular problem, like the antarctic whale fishery, or restricted to a limited area of the ocean.

Hence it is interesting to see in what ways the latest series of oceanographic reports resembles, and differs from, its predecessors. The Carnegie Reports, whose most recent numbers are a monograph on the genus *Ceratium* and a report on the bottom samples, are published in the large quarto form established by the *Challenger* reports more than sixty years ago, but the printing inside the covers is quite different. Instead of fine large type in long lines across the page, there is a double column of photolithed typewriter text, with jagged right hand margins. Perhaps there are economic advantages in this, and it must be said that the method produces satisfactory illustrations at comparatively low cost, but it is hardly a delight to the eye. Nor is it very restful to read, for the unfinished appearance of the margins exaggerates the blurring which too often occurs in this type of reproduction From the viewpoint of the appearance of the series as a

whole, it is difficult to understand why this method was adopted after the first two reports (on copepods and *Tintinnoina*) had been printed in the more formal and legible type.

However, the important thing is that the results are being published. They reveal the change in emphasis which has occurred in oceanography since the days of the *Challenger* and the *Albatross.* That emphasis is now on precise physical and chemical data, and comparatively little biological collecting, except of plankton, is now done. The *Carnegie*, of course, was not equipped for heavy dredging, and its work represents the extreme departure from the earlier program of oceanography. Nevertheless, the plankton reports of the Carnegie cruise are especially valuable because of the detailed physical data which are correlated with them, data which earlier expeditions could not gather because of cruder instruments. It is not enough to know what animals occur in the ocean, or even where; we must also know when, and in relation to which conditions, and such reports as these on the dinoflagellates and copepods are long steps toward the synthesis of hydrography and biology which is still the primary goal of oceanography. It is with regret, however, that this reviewer records this change in the order of things, for he misses the elaborate faunal monographs which were formerly the bulwark of every series of reports. As for physical data, the Carnegie reports leave little to be desired, and in several instances data from other recent expeditions and surveys have been included. An especially valuable feature of the report on bottom samples is the detailed station list with descriptions of samples from nearby stations occupied by earlier vessels, notably those of the *Challenger* and *Albatross.*

In the past an Antarctic expedition has contributed much to oceanography and marine biology, and those lately completed by the British and colonial governments have been no exception, but the second exploring expedition to be authorized by Congress has little to offer to these fields of knowledge other than a paper on Ross shelf ice and some reports on birds, general descriptions of antarctic biology and miscellaneous identifications of marine invertebrates. Unlike the Wilkes expedition of 100 years ago, which was the other authorized United States exploring expedition, the Byrd expedition was devoted primarily to land exploration and geology instead of oceanic exploration. This symposium volume of the Philosophical Society, which apparently embodies all that is to be published on the expedition, contains several noteworthy papers on antarctic geology and geography, and some detailed meteorological observations. The bulk of this work was done on the Palmer Peninsula, and is further located by a series of names which are evidently based on the Dunn and Bradstreet rating of the sponsors, for Congress, as usual, failed to appropriate sufficient funds. It will be interesting to see what names are finally applied to these various features, since English explorers have their own set of names for Graham Land, as they call this region. However, there is something liquid and musical about such a name as Mobiloil Bay. The volume is illustrated with excellent, well-produced photographs which will remind many of a well-known geographic magazine.

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CHEMICAL DETECTION

Discovery of the Elements. Mary Elvira Weeks. 578 pp. Fifth ed. Illus. 1945. $4.00. Journal of Chemical Education, Easton, Pa.

THE appearance of a revised fifth edition of Dr. Weeks' well-known *Discovery of the Elements* is sufficient evidence of the growing popularity of this work not only among chemists but also with those who are interested in the general history of science. The first edition of the book, compiled in 1933 from a series of articles by Dr. Weeks in the Journal of Chemical Education, has expanded in the course of a few years from a volume of 363 pages and 875 literature references to the present work of 578 pages and 1599 citations; or, differently stated, from a book of 21 chapters and 282 illustrations to one of 27 chapters and 347 illustrations.

Among the new features of the present edition should be noted the list and page

numbers of the many interesting illustrations, the great majority of which are from the large collection of Professor F. B. Dains of the University of Kansas. Additional biographical sketches have been inserted and new information has been made available, particularly upon the history of developments in the countries of the Western Hemisphere. Each chapter is preceded by a brief summary of its contents and by an appropriate quotation from one of the chemical classics. The anecdotal treatment, employed by Dr. Weeks in the previous editions of her book and followed in the present work, gives much of human interest to the unfolding of the story of how each of the elements was discovered—a story which the discoverer, wherever possible, is allowed to tell in his own words.

The book can be enjoyed as much for random as for continuous perusal and can be dipped into with equal pleasure by the young and old. For the beginner in science, there are few books more suitable for collateral reading than the present work with its fascinating and stimulating accounts of how perseverance, or intuition, or accident, has contributed to the making of some discoveries and of how lack of caution, or over-caution, has prevented some investigators from achieving the fame of discovery. To older readers the book will be a veritable treasure-trove of pleasing scientific information gathered by Dr. Weeks in her diligent search of ancient records. The wealth of literature references at the conclusion of each chapter will be a useful guide to librarians and to those desirous of enriching their collections with the rarer works on the history of chemistry. The typography of the new volume is excellent, the reviewer having noted only the peculiar syllabic transposition of "inNasi" for "Nasini" on page 340.

Because of war-time paper restrictions this new edition of Dr. Weeks' *Discovery of the Elements* is announced to be a limited one. It is hoped that the publisher has allowed for its probable popularity indicated by the sale of previous editions.

C. A. Browne

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Washington, D. C.

EARTH SCIENCES RE-EXAMINED

Principles of Physical Geology. Arthur Holmes. 640 pp. Illus. 1945. $4.00. The Ronald Press Co., New York.

Elementary textbooks are so characteristically pedestrian and dull that it is a relief to come upon one which maintains a high level of interest for more than 500 pages. Geology is a fascinating subject, but the penchant of most teachers and textbook writers to reduce every aspect of the subject to definition and to routine exposition deadens the interest which normal underclassmen exhibit in college courses that preserve some spontaneity and excitement. Arthur Holmes imparts much of the excitement which the subject of geology evidently arouses in him.

The effectiveness of the volume must be attributed in large part to the unity which has been achieved in the development of the subject. A single author has a distinct advantage over collaborating authors in this respect, both because he can maintain better perspective in preparing the several parts of the book, and because, by use of a recurrent theme, he can hold the parts together and even dramatize them. Holmes has made the most of these advantages and has coupled with them the benefits which accrue from good writing. Furthermore, the student who uses the text is likely to be infected with the idea that there are many intriguing problems still to be solved in the earth sciences and in many parts of the earth.

Starting with a rather long preliminary survey of the earth, the materials composing it, the processes at work upon it, and the geologic modifications which these processes effect in the course of time, the author then gives an efficient account of the external agents, wind, water, ice, and life. Noteworthy are the two chapters devoted to the work of running water, if only because they present geomorphic views which are firmly established in Europe, but which are still considered controversial in this country. In contrast to the excellence of these chapters, the treatment of groundwater is more disappointing than it is in most elementary texts. It is obvious that British geologists have but little more than a reading acquaintance with solution phenomena and the solution cycle.

Holmes' chief interest lies in the operations of the internal geologic processes. The minutiae of structural and igneous forms, to which American geologists are prone to devote an inordinate amount of space, are quickly disposed of in the preliminary survey, and in the part assigned to the internal processes the author plunges into the broad aspects of diastropism and igneous activity. His method involves a survey of the major landforms of all the continents, and even an instructor will lay the book down with a better integrated knowledge of the earth, however much he may disagree with some of the author's hypotheses and conclusions.

It is easy to find fault with such a book. The American geologist will wince at the vague and, at times, erroneous references to American geography, forgetting his own imperfect knowledge of European geography in general and British geography in particular. From the practical standpoint of the classroom teacher, however, a book which draws most of its illustrative material from Europe cannot be used very effectively among American students, who know altogether too little about their own country.

Criticism will undoubtedly center upon the meager space which Holmes devotes to rocks, minerals, and structures; to the brevity with which some other subjects are dealt with; and to the wretched reproduction of the well-chosen photographs. Between them the reviewers have had 35 years' experience in elementary instruction, and they are prepared to defend Holmes in his rejection of certain details which can be more effectively taught in the laboratory than in a text or in the lecture room. Most of the textbooks in current use kill student interest in minerals, rocks, fossils, and structures by setting down interminable definitions and diagrams which, to the student, become so many exercises for laborious memorization. Spontaneous interest in the subject is given only a meager chance of survival beneath the avalanche of deadly detail. For this reason the reviewers are more than willing to applaud the omissions, which are obviously judicious and not inadvertent.

For the American undergraduate who is taking his first course in geology, *Principles of Physical Geology* is not an entirely satisfactory text because it assumes a background of mathematics, physics, and chemistry which too few of our students have. In consequence, some of the chapters in Parts I and III are somewhat beyond the reach of the average freshman and sophomore. For an advanced course in physical geology the volume should prove valuable in providing fresh illustrative material, but as a text in such a course, the volume is rather elementary. It should find most effective use among instructors who wish to freshen their viewpoint and among nongeologic scientists who want a knowledge of the subject. For both groups Holmes' book is required reading.

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COMMENTS AND CRITICISMS

REACTIONS TO LEAKE'S "ETHICOGENESIS"

THE effort to bring ethics into an immediate causal alignment with knowledge, to make of reason the "good," has everywhere occupied the minds of profound thinkers. Dr. Leake's views on ethicogenesis, in keeping with this tradition and modified by the convention of the modern era, assume the possibility of such a relationship between science and ethics; assume that man need only learn from science what is "good" for him in order to construct promptly for himself an ethical system out of his knowledge. That this pattern has not been followed, at least down to the present, is indicated by the glaring discrepancy existing between the comparatively high level of human knowledge and the still-predatory level of civilization. There is no dearth of historical evidence to show that knowledge reaches the level of practical and ethical attitudes only after the lapse of considerable time; as much time as is required for that knowledge to filter or buffet its way through the barrier of prevailing practical and ethical attitudes. That is to say, knowledge reaches the ethical level only after it has undergone a process in which opportuneness plays the major role; a process related to the interaction of the material and intellectual factors of an era. On this view, ethics, as a product of many factors of which knowledge is but one, appear to be an accretion upon and a phenomenon relevant to the phenomenon of human society.

Ethics are unique in the biological world and, more than any other single characteristic of man, distinguish him from his evolutionary forbears. If the difference between the behavior of man in his social environment and the behavior of lower organisms in their natural environment is merely a matter of degree, as is often affirmed, it is necessary out of scientific scepticism to determine whether or not this difference in degree is sufficient to produce qualitative differences between them. It is a far cry from the "ethics" of biological symbiosis to the self-abnegation of Judeo-Christian ethics. A teleological view of nature might consider both, as kinds of relationship, to be analogous ethical devices; yet it is impossible to measure the survival value of both by the same meaning of the term survival. In its social context survival is often conditioned so that it may appear to modify or even reverse biological law. Here then is one qualitative factor barring the direct application of biological knowledge to ethics. The *amount* of knowledge possessed by man about himself does not necessarily, by its magnitude alone, total up to that precise knowledge required to eliminate his lethal conflicts. The task of science with ethics is the selection and orientation of that knowledge which will enable it to span the qualitative gap between society and the natural world; to create, so to speak, a medium of exchange between them. To my knowledge, except for the figurative abstractions of philosophy and poetry, no valid biological correlate of the phenomenon of ethics has been established. On the assumption and evidence that man is subject to biological law, his ethics must necessarily be found to have a place in the biological studies of man. I have no doubt that this is possible, but it seems necessary first to read ethics in terms of their social root-source before the problem of ethics can be made commensurate with the method of science.

Moving directly, by figurative analogy, from biological phenomena to society, Dr. Leake loses sight of the fact that the survival of the individual or of individual groups or of the relationships existing between them is no longer ethically relevant in an environment where the survival of a part is dependent upon the survival of a unit beyond itself and by which it is encompassed. In his social context man is no longer a biological unit, and ethics based upon survival value apply no longer to him directly but to the survival of the greater unit. Dr. Leake's principle of harmony in groups adds a foot to the toes for better function, but it seems to do little for the total organism, society. In our present era there is a guarantee written in anguish and degradation that the ethics of the total social level take precedence over isolated individual or group ethics. The physical unity of the world requires nothing less than a total ethical principle, an ethic which, as a social force, will have the power to invite the efforts of science for the welfare of society and the individuals who are its creators and its product.—ANNE ROSENBERG.

Chauncey D. Leake, in his profound search for a law of Ethics (in THE SCIENTIFIC MONTHLY, April 1945) devotes a few remarks to the origin of the so-called Golden Rule. My training in the Jewish faith taught me that what he calls the Buddhistic form of the Golden Rule actually was the reply of Hillel I (died A.D. 5) to the question of a heathen about the teachings of the Hebrew religion. I always held this answer to be genuine, a belief which is strengthened by the fact that the famous phrasing

of the Golden Rule in Matthew is followed by the explanation: "for this is the law and the prophets." We, therefore, must conclude that both the affirmative and the negative formulation of the Golden Rule, were popular expressions in Palestine at the time of Jesus, and no sharp distinction can be drawn between them, as the author seems to think.

The exhortation: "Thou shalt love thy neighbor as thyself," which is often identified with Christian teachings, is wrongly translated from Leviticus XIX, 18. It implies that self-love is the strongest expression of love conceivable, an idea alien to the ancient mind. What the lawgiver wanted to impress is simply: "Love your neighbor, he is like you." Many modern translators, e.g., Buber and Rosenzweig, have recognized this. The fact that the Hebrew language has no verb for "to be" led to the century-old misunderstanding.—L. SELIGSBERGER.

To read your "Ethicogenesis" in the current SCIENTIFIC MONTHLY was thrilling to me. It was like a cool breeze on a hot day. So clearly and beautifully set forth, and so inspiring in its implications, your exposition of rational ethics has placed us all under obligation to you.—ALVIN L. DAVIS.

I have read conscientiously the article "Ethicogenesis" by Dr. Chauncey D. Leake to which you assigned first place in the April issue of your magazine.

The attempt to give ethics a scientific foundation seems to me most praiseworthy since the authority for making good use of knowledge must come from knowledge itself and not from superstition.

What I do not understand, however, is why the *good* must be identified *scientifically* with that which has survival value for the individual or the species, since the test of scientific truth has never been its survival value for the scientist or his human species.

I notice also that it does not require a tedious analysis to discover that if the good is *scientifically* identified with survival value, the net result is that Science is made the intellectual handmaid of Machiavellism. This is so because sometimes it is honesty that has the survival value, but too often survival for the individual or his group lies the way of fraud, deception, deceit. It was looking at the survival value straight in the face that Machiavelli warned his Prince that the practice of virtue is a risk, but the appearance of virtue is safe and commendable. Looking at the survival value he recommended as "useful vices" cruelty, hypocrisy, etc.

Then, too, I doubt very much whether even an ethics for the species could be evolved on the survival value idea. "Survival is 'good,'" Dr. Leake says, "in the very significant sense that if the species fails to survive, 'goodness' has no further meaning for that species." But goodness can have no meaning for such an abstract concept as "species." Any student of semantics would tell us that much. Goodness has meaning only for the individuals constituting the species. Now no individual can be expected to risk his survival in the interest of the species since for each individual the species dies with him and goodness for him is coexistent with his own survival. When he is dead, Dr. Leake reminds us, goodness has no further meaning for him.

Scientific ethics must be something above and beyond this survival value concept which in my estimation debases both ethics and science. A scientific ethics must be an ethics based on the nature of man, not on the conditions for survival. Science could tell us, for instance, that the authority for the *ought* must rest on the nature of man's being. A stone, science could tell us, has no categorical imperative to act as a bee, nor a rat as a man. Science could make it very plain that independent of the *is* the ought is meaningless. And the answer to the question, what *is* the nature of man is certainly science's jurisdiction.

It can be scientifically determined either by historical observation or by laboratory experimentation that man has a double nature:

1. Independent of all nurture, man has an affinity for things, which lies back of his specific urges to grab and to hold, and which spells historically class struggle and international wars.
2. Independent of all training, just by pure nature, man has an affinity for correct solutions, just solutions. (A just solution differs from a correct solution merely in that in the case of the just solution the factors of the problem are sensient beings.)
   This affinity for right solution has produced both science and philosophy.

It can be proved scientifically that man has one part of his personality which is not satiated with things but with correct solutions. And it is on this condition of man that ethics has its true scientific foundation.

For the side of man's personality which is not satiated with things, Machiavelli has no message. Hypocrisy has no appeal for this side. As man's stomach hunger is not placated by a cardboard chicken, so this other hunger for correct solutions is not placated with the appearance of right doing.

We have plenty of historical witnesses testifying to the fact that this part of man's nature is no 20th Century addition, that it had always been there. This portion of his personality is what the Greeks glanced at and called Intellect, what Kant peeped into and called Reason, what Christ scrutinizingly observed in all its potentialities and called it the kingdom of heaven—the kingdom of heaven within man, small as the grain of mustard seed.—ANA MARIA O'NEILL.

THE BROWNSTONE TOWER

Occasionally we receive indignant letters asserting that certain articles we have published do not belong in the SM because they are unscientific propaganda. Our experience shows that such letters are always directed against essays involving human behavior and human problems, and we suspect that our correspondents' indignation has been aroused not so much by the character of these essays as by the views expressed in them. Nevertheless, we are caused to reflect on the possibility that we may be allowing propaganda to creep into the SM.

We could seal the SM against social, political, and religious propaganda by restricting it to the natural sciences. If this were done, we should not only be avoiding our obligations to sections H, I, K, L, and Q of the AAAS but would be doing more harm to the SM than good. We must not expect essays in social science to be mathematically exact and we must bear in mind that in the field of human relations, which interests everyone of us humans, scientists cannot always be scientific; indeed they are usually unscientific. Take, for example, the assertion just made. Is it true? Consider the elaborate and time-consuming philosophical and statistical investigation that would be needed to answer this question; that is, to give scientific validity to our assertion. It is not feasible to confirm the apparent truth of many statements made by scientists on human problems. Their assertions, decisions, and recommendations are based not only upon statistical information but upon experience. To the reader the truth of such dicta must be judged by the extent of their agreement with his own experience. We believe that appeal to general experience in lieu of factual numerical data is entirely justified, for, if scientists refused to express themselves except on numerically predictable subjects, they would leave their fate and that of their fellow men entirely to politicians.

If our reflections are reasonable, we should continue to publish articles on social problems and perhaps to increase the proportion of space devoted to them, not only because of their intrinsic interest but because scientists wish to take greater social responsibilities than they have done in the past. How shall we decide what manuscripts to accept? Let us discuss this question by means of examples from the August issue.

Our leading article by Emeritus Professor A. G. Keller of Yale was on compulsory military training in peacetime, a question of national importance that cannot be answered in the laboratory. We accepted Professor Keller's essay because (1) it was timely, (2) it was well written, (3) it represented the views of a man who was qualified by a lifetime of professional experience to make recommendations on this question of public policy. Because he advocated compulsory training, we knew that some of those opposed would cry propaganda. But Professor Keller cannot be a propagandist. He is retired, has no ax to grind, and speaks up only as an individual scientist and citizen who is concerned about the welfare of his country.

On a controversial subject we think it is better to publish a separate article on each side of the question than to print an apparently scientific and dispassionate article in which the pros and cons are neatly balanced without recommendations. Sometimes we can publish on different aspects of a subject in the same issue. Thus, in the August issue we published Dr. F. B. Sumner's magnificent essay on old age and death—the orthodox point of view of a biologist. Then to show that physical scientists and engineers often have a different outlook on life as old age approaches, we published an unusually pertinent sermon by an engineer, David Moffat Myers, who spoke "the gospel truth." More often we must give a hearing to the opposition in a subsequent issue. Thus, in the present issue the opponents of Leake's "Ethicogenesis" and of Keller's compulsory service present their arguments.

We recognize the danger of accepting unsound manuscripts when scientific standards do not and cannot operate. But we would rather take that risk than shun controversial subjects. We hope that our own opinions on such subjects do not influence our decisions on acceptability of manuscripts; we insist only that the writer express himself clearly and reasonably and with some originality. If we make errors of judgment in our selections, we have some consolation in the thought that misguided and erroneous papers have often done much good by arousing readers to action for the sake of truth.

PARTLY for its intrinsic interest and partly as an illustration of cosmopolitan social wisdom from a scientist who is not afraid to express himself, we publish on page 317 of this issue the thoughts of Dr. Howard A. Meyerhoff, a geologist, on certain provisions of the Report of the Berlin Conference, which will affect the economy of Germany and surrounding countries.

Since July of this year Dr. Meyerhoff has been our nearest neighbor in The Brownstone Tower. Temporarily on the floor above, he comes into our office periodically to sharpen his pencils and his wits. It is time that our readers knew him as the new Executive Secretary of the AAAS, not taking the place of either Dr. Moulton or Mr. Woodley but doing important work that neither of them was free to cultivate. If we were to call Dr. Meyerhoff the Diplomatic Secretary of the AAAS, the reader might guess what he does. Are the bonds of affiliation between the AAAS and other societies in need of attention? Dr. Meyerhoff will strengthen existing bonds or provide new ones. Does pending legislation concerning science and scientists need scientific guidance? Dr. Meyerhoff will provide it. To paraphrase it, he supplies power and oil for the machinery of the AAAS.

Dr. Meyerhoff is on leave of absence from Smith College where he made his reputation in geology and in public service. It was his outstanding performance as Secretary of Section E of the AAAS that caused him to be selected at the age of 46 for service to the Association as a whole. We will not list Dr. Meyerhoff's professional connections nor his services as a public-spirited citizen nor his experience in business and government. It is enough to say that he knows his way around in the world. We are happy to have him with us and hope that he never gets a pencil sharpener of his own.

YOU, kind reader, have received from the Office of the Permanent Secretary a leaflet explaining the need of the AAAS for a home of its own in Washington and appealing to you to contribute to the centennial building fund. Twice before in his scientific career the editor has worked for a new building, but never in his experience has a new building been so necessary as it is to the AAAS. Present personnel is strained to the breaking point by the work that must be done for the Association's enlarged membership. Because space is lacking, additional personnel cannot be brought into present quarters. We have standing room only.

In stressing the gravity of the situation the editor is not speaking for himself. His working facilities are better than those of any other member of the staff, and if he had an assistant editor in another room in The Brownstone Tower, he would be content to remain in the Tower indefinitely. But he can get neither the room nor the assistant, which he must have someday, somewhere, if the SM is to grow with the Association.

Consequently, we must all leave the Smithsonian Institution, but we hope that we shall not have to go far away from its inspiring environment. We are now on the greatest campus in the world, the Washington Mall, which we cross twice a day between the National Museum and the Smithsonian Institution.

To the west stands the glorious Washington Monument, to which our eyes never fail to turn in admiration. We have seen it in many aspects: standing sharp as a sword against an azure sky or mottled with fleeting cloud shadows or shrouded in mist like an enormous ghost. But one night not long after VE-Day we saw it framed in a most memorable manner. We descended the Tower elevator and stepped out into a dark room. Facing the elevator was a narrow window behind which stood the Monument, illuminated again after years of blackout. Our spirits soared to the apex of its gleaming aluminum tip. What a symbol of the highest and purest human aspirations!

At the east end of the Mall stands the Capitol, now also illuminated at night. But we like to think of it as it appeared late one summer afternoon when it was surmounted by a rolling, towering, luminous thunderhead, which gave it superhuman significance.

From our office at night we can see that the light shines again on the great statue of Lincoln in the Memorial. Again facing reconstruction following the devastation of war, we think of Lincoln's second inaugural address carved on the wall of the Memorial: ". . . let us strive on to . . . do all which may achieve and cherish a just and lasting peace among ourselves and with all nations."

We *must* leave The Brownstone Tower, but when we depart for the last time let it be on a windy night with ragged clouds blowing across the face of a full moon behind the Tower.

—F. L. CAMPBELL